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The Principles of Systematic Entomology

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#4541

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PREFACE

As far as I am aware there exists no single book to which one who may be interested in the problems of systematic entomology may turn for a discussion of the principles upon which such work is based and a consideration of the methods by which those principles may be translated into practice. It is quite true that all these things have been discussed from time to time and at more or less length in our various entomological texts and journals, but they appear never to have been assembled into a comprehensive treatment.

It is primarily as a discussion of these fundamental principles and of the philosophical background of systematic entomology that this volume is presented. However, to give concrete reality to any such philosophical discussion it is necessary also that the means by which these principles may be applied shall also be considered, to some extent at least. Consequently, although this book is not intended as a manual of detailed methods, there will be found a general discussion of them, more or less detailed in the case of things which have not been extensively treated elsewhere. As a matter of convenience for systematists there is included a reprint of the International Code of Nomenclature.

This volume is a frankly critical survey of the existing conditions in systematic entomology. As such, it is not to be expected that there will be general agreement with everything that is said. It is my conviction that the study of insects is at present in a transitional period, passing from a stage where it has partaken of much of the characteristics of a hobby, with the butterfly-collector and his net serving as its symbol, into another and far more exacting period in which it is rapidly coming to be recognized as a distinct and definite profession. As such it will demand of its followers a training as thorough and as rigid and scientific standards as high as are required in any other profession. At the present time, however, it is a curious combination of two things, a profession and a hobby of dilettantes.

In this developing profession the work of the systematist must have a place, for it cannot be dispensed with. And his work will undoubtedly be subjected to increasingly more exacting and rigid demands if it is to fulfill its function and to justify itself as worthy of respect. It must grow, not merely in volume, but in its fundamental values, in the breadth and the depth of its philosophical concepts, in the thoroughness of its practice, in the validity and trustworthiness of its conclusions.

And so it seems that this is a peculiarly fitting time at which to attempt such a critique of systematic entomology as is here presented.

WITHDRAWN

I wish here to acknowledge my indebtedness to various colleagues who have assisted in the preparation of this volume. Mr. J. C. Chamberlin has permitted me to draw freely from an unpublished manuscript dealing with the matter of generic concepts and has discussed with me many of the ideas that are here presented. Professors E. O. Essig, S. B. Freeborn, and E. C. VanDyke, of the University of California, have read parts of my manuscript and have offered suggestions and criticisms that have been of very decided aid, especially in those things in which they have disagreed with my views.

G. F. F.

Stanford University
June 15, 1928

TABLE OF CONTENTS

PAGE

List of Illustrations 7

CHAPTER

I. The Contribution of the Systematist to Biology 9

II. The Scope of Systematic Biology 18

III. The Principles of Systematic Entomology 24

IV. The Segregation of Species 36

V. Categories Less than the Species 51

VI. The Morphological Basis of Systematic Entomology 58

VII. The Preparation of Material 66

VIII. Entomological Drafting 76

IX. The Description of Species 100

X. Classification 108

XI. Nomenclature 128

XII. The Training of the Systematist 158

Index 165

LIST OF ILLUSTRATIONS

FIG. 1.—Diagrammatic representation of different methods of employing divided drawings for bilaterally symmetrical objects..... 86

FIG. 2.—Diagrams showing the effect of conventionalized shading as employed by mechanical draftsmen..... 88

FIG. 3.—Simple types of shading..... 89

FIG. 4.—Methods of drawing setae..... 90

FIG. 5.—A Nycteribiid, *Basilia antrozoi* (Townsend), illustrating the application of the methods herein described to the figuring of a flattened, but rather complex, object..... 92

FIG. 6.—*A*—A Mallophagan, *Trichodectes bovis* (Linn.), illustrating the use of pure outline. *B*—A Mallophagan, *Goniocotes gigas* Taschenberg, illustrating the use of a simple type of shading where it is desirable to indicate degrees of pigmentation..... 93

FIG 7.—Slight modifications of the outline drawing as applied to the pygidium of a scale insect, *Chionaspis salicis* (Linn.)..... 95

FIG. 8.—Application of the methods herein described to a rotund and complex object, a beetle of the genus *Rhynchites*..... 96

FIG. 9.—Wing of beetle of genus *Rhynchites*..... 98

FIG. 10.—Selection of drawings illustrating technique of a skilled entomological illustrator 99

FIG. 11.—Graphs illustrating the "hollow curve of distribution"..... 115

CHAPTER I

THE CONTRIBUTION OF THE SYSTEMATIST TO BIOLOGY

It is probably safe to say that in point of years of service systematic biology is the senior branch of the whole field of the study of living things. It can hardly be otherwise, for discrimination among or between facts or things is one of the very first steps in the organization of our knowledge of these things or facts and our use of them. In whatever field of activity we may engage this basic process soon becomes necessary. To discriminate among those things which are bad and those which are good, those things which are useful and those which are not, to place together into some sort of system those things which seem to have some kind of connection or relationship or at least some degree of likeness one to another, these are acts which come almost automatically as soon as the human mind begins to busy itself at all with actually thinking about its surroundings.

All of this is particularly true as applied to what we call Science. As Professor Raymond Pearl[1] has said: "The first step toward a proper knowledge of the phenomenal world is obviously to get the phenomena classified in an orderly scheme. In biology this takes the practical form of getting the different kinds of animals and plants described, named and classified." Or, as an entomologist[2] has expressed the same thought: "Objects without names can not well be talked about or written about; without descriptions they cannot be identified and such knowledge as may have accumulated regarding them is sealed; unclassified their relations are unknown and the possibilities of deduction are destroyed."

However, it is only when the mind has reached a rather high stage of development that anything more than a purely artificial system of classification is arrived at. The undeveloped and the untrained mind may—and almost invariably does—group together things whose similarity is merely superficial. Plants are grouped together merely because they are trees, or vines, or shrubs. Animals are grouped together merely because they live in a common environment or because they have a common habit. Ideas are grouped together although they may have no real connection with each other. It is only when the definite thought of searching for significant rather than merely apparent differences and resemblances occurs that we have the beginnings of what is at least in spirit and intention a *scientific* classification.

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[1] Pearl, R., "Trends of Modern Biology," *Science,* n. s., LVI, 583, (1922).

[2] Gahan, A. B., "The Rôle of the Taxonomist in Present Day Entomology," *Proceedings, Entomological Society of Washington,* XXV, 68–78, (1923).

This failure to achieve, or even to attempt, a truly scientific classification of facts and of ideas is conspicuous in the history of the development of human thought. In fact the whole conception of such a classification is really a very modern thing. It is consequently not at all surprising that this conception has failed to achieve general and unqualified acceptance outside of Philosophy and the various branches of what we know as Science. In fact it may be said that the greatest of all the contributions which Science has made to the development of human thought is its insistence upon this very idea.

And yet it may also be said that in but very few fields of knowledge, even within the boundaries of Science, have the available facts and the available ideas even yet been reduced to a thoroughly scientific system, that is, to a system from which every conclusion that is influenced by prejudice, or preconceived ideas, or false information, or wrong thinking has been rigidly excluded. It appears that the development of such a system in any field is possible only in an extremely advanced stage of thought, a stage to which probably the majority even of those of us who call ourselves "scientists" have not yet completely attained.

In biology the attempt at something of a scientific separation and grouping of the facts having to do with the number and the relationships of the various kinds of organisms has had a very respectable history. More than two thousand years ago Aristotle made some attempts at a classification which was in spirit at least scientific. During the long period of the middle ages he was accepted as an authority by those whose mental shackles precluded them from improving upon his work or appreciating the significance of his method. As we approach the times that we like to call "modern" the interest in the study of the different kinds of animals and plants began to increase. It may very well have been that it was stimulated by the great numbers of strange things that were revealed by the great explorations. So it was that about two hundred years ago the matter of the separation, description, and naming of species became the major part of the study of organisms. With the impetus given by the Linnean invention of that most useful tool, the binomial system of nomenclature, this describing and naming and classifying assumed a tremendous importance.

In fact it may be said that there was a period during which this kind of work constituted almost all there was of biology and its most conspicuous feature was the giving of names. That this should have been so was entirely natural, for the great explorations had brought to light hosts of forms for which no names existed and of which it was consequently difficult to speak. Furthermore, the idea of doing more than merely giving

names and arranging the various species into some sort of system had scarcely taken root. The modern branches of biology that involve the use of experimental methods were scarcely thought of. Even that fundamental discovery of the cell, without which biology as we know it would be almost inconceivable, came only as late as 1838, and from that time dates the greater part of the tremendous development of the last hundred years.

To quote again from Pearl:

> It may fairly be said that up to the time of Wallace and Darwin and the "Origin of Species," all biology busied itself with the answering of one phase or another of the following two naïve questions.
>
> *First,* how many and what different kinds of animals and plants exist, or have existed, on the face of the earth.
>
> *Second,* regarding living animals and plants as ingenious and complex contrivances, but after all not fundamentally unlike other contrivances, how are they put together and how do they work?[3]

We may add to this the remark that the answering of the second question, at least as it refers to how organisms are put together, was in no small measure the work of those who were interested primarily in the first question.

Since that time, however, the other branches of biology have developed and the relative importance of the systematic branch has declined. It is no longer all or even any preponderant part of biology. It is—as it should be—merely one of a long series of branches. Its importance among those branches is a matter of opinion. There are those who would consider that it represents merely the "seed leaves" by which a young plant is nourished, but which are finally sucked dry and cast off. And there are those who, like the present writer, believe that it is still one of the sturdiest branches of the tree, with room beneath its shade for all sorts of highly important and profitable activities. Its first exuberance of growth, its first freshness of spring, is perhaps past, but the sap still flows within it. It may be encumbered with dead twigs and rotten branchlets, but it can still produce fruit.

In view of this difference of opinion as to the actual value of systematic work it may be well to consider something of what its contributions to biological thought have been.

Undoubtedly the mere naming and superficial describing of species, the piling up of veritable mountains of them, was in itself of value. Through it men came to know of the richness and diversity of life. They came to know at least a part of what an enormous assemblage of forms live and

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[3] Pearl, *op cit.*, p. 582.

have lived upon the earth. This fact alone is of very marked significance in the history of human thought, for it alone was sufficient to cast doubt upon the validity of certain long held and carefully cherished conceptions. It were out of place here to enter into a detailed exposition of the conflict of which all this is a part and to revive again controversies that scientific men had, until recently, come to believe entirely settled, but anyone who is interested will find them discussed at length elsewhere.[4]

On the other hand, the work of systematists has shown with equal clarity the essential unity of life. Extraordinary as the diversity of life may be there runs through it all a unifying thread. The vision of this thread and the recognition of this essential unity has had its expression in the development of the Theory of Evolution. There is no need here to enlarge upon or attempt to explain the influence that this theory has had upon modern thought. In effect it has wrought a revolution in every field of knowledge. The point of view of philosophy, of religion, and of every branch of science has been profoundly affected by it.

The development of the Theory of Evolution was essentially the work of men who were primarily systematists. It was the contemplation of the phenomena associated with the numbers and the differences and the distribution of species that led to the attempt to explain their origin. It was the contemplation of the same phenomena which furnished at least the greater part of the evidence in support of the explanation that was developed by Darwin and Wallace. And even though the explanation of the fact of evolution which was offered by these two men be as unsatisfactory as the most extreme representatives of the most modern school may contend, it appears to the more judiciously minded representatives of these schools that the work of the systematists is not thereby discredited.

Says Bateson: "If we cannot persuade the systematists to come to us, at least we can go to them. They too have built up a vast edifice of knowledge which they are willing to share with us and which we greatly need. They too have never lost the longing for the truth about evolution which to men of my date is the salt of biology, the impulse which made us biologists. It is from them that the raw materials are to be drawn which alone can give breadth and catholicity to our studies."[5]

And says Pearl, who is not a systematist and whom no one can accuse of special bias in favor of the systematists:

It is the systematist who has furnished the bricks with which the whole structure of biological knowledge has been reared. Without his labors the

[4] White, Andrew D., *A History of the Warfare of Science with Theology,* I, 44.

[5] Bateson, W., "Evolutionary Faith and Modern Doubts," *Science,* n. s., LV, 60, (1922).

fact of organic evolution could scarcely have been perceived and it is he who today really sets the basic problems for the geneticist and the student of experimental evolution.[6]

The problems associated with the distribution of organisms have attracted much attention. This has been so not merely because of an intellectual curiosity concerning the matter of distribution itself, however much that may merit consideration. Upon the questions of distribution hinge certain clues to the answers of other problems in entirely different fields, notably, for example, the problem of the distribution of ancient land masses. The question of how the various species and larger groups have come to be distributed as they now are upon the earth is closely connected with the problem of how the land masses of ancient times were distributed. And so the student of paleogeography relies to a very high degree upon the results of systematic studies. He has perhaps at times been entirely too willing to move continents about, to drain oceans and to level mountain ranges at the behest of the systematist, but such mistakes merely indicate that he has not used his judgment sufficiently. They do not militate against the value of the systematists' evidence when properly interpreted.

It should be noted that in such matters the validity of the conclusions arrived at is entirely dependent upon the quality of the systematic work upon which they are based. It were sad indeed to construct a continent or to drain an ocean or to build a mighty land bridge only to find that such labors were entirely unnecessary had the systematists done their work with sufficient care!

The problems of paleontology and stratigraphy are almost exclusively problems of a systematic nature and the contributions of paleontologists to our body of knowledge and theory are almost purely contributions drawn from and based upon systematics. That these contributions are not small anyone who is at all familiar with the development of evolutionary theory will agree. The proof of the succession of forms throughout the ages, the evidence of the rise and decline and even complete disappearance of whole great groups of animals and plants, the clear evidence of the working out of evolutionary processes, all these are the contributions of systematists.

The contribution of paleontology to the knowledge of ancient land forms and to the knowledge of the shifting of land and sea masses and of ancient climates is again almost purely dependent upon conclusions drawn from the systematic study of fossils.

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[6] Pearl, *op. cit.*, p. 582.

The ever fascinating problems of human origins and of human races are nothing more than problems of a systematic nature. Whether the Piltdown man was a man or a chimpanzee, whether the Trinil remains are those of a man-like ape or an ape-like man, these are purely systematic problems. The physical anthropologist is merely a systematist, working toward a specialized aim and with the aid of highly specialized and refined methods.

Even in modern genetics, however much the thorough-going geneticist may wish to deny the statement, the need of purely systematic studies is not lacking. For after all, what is the separation and recognition of the various mutants with which the geneticist deals but a simple problem in systematics?

One might go on indefinitely in thus recording the contributions of the systematist. What has been said is merely a summary review of the subject. It should be enough, however, to reassure anyone who feels that perhaps such work is unworthy of the serious efforts of one who wishes to be known by the proud title of "Man of Science."

It is probably true that no one will again develop from systematic work a theory as potent as that of evolution. Probably no systematist may ever again aspire to be another Darwin. Nor may any geographical explorer again aspire to be another Columbus. Even so there remains much to be done. The fact that the great continents of thought have been quite definitely charted does not mean that the days of exploration are ended. There are still islands to be examined, rivers to be followed to their sources, mountain peaks to be scaled, deserts to be penetrated. There is still a place for high endeavor.

All that has so far been said has to do with the philosophical aspects of biology. There is another series of contributions that have come from systematic work and that have to do with purely practical considerations.

Discrimination among species is the basis of systematic work. It is also the starting-point of practically all applied biology. Consequently systematic work has a very definite function in even the purely practical branches. Examples to confirm and to illustrate this contention may be found everywhere in biology, but as this discussion is concerned primarily with entomology we may be satisfied with taking examples from our special field.

There is of course the simple fact, to which we have referred earlier, that without a name and without means of identification it is impossible to correlate knowledge and to know what may have been learned by others in regard to any particular species. This is a circumstance so obvious

that its importance is frequently overlooked. We may here take it for granted as sufficiently established to need no supporting argument.

There has developed in recent years the field of economic entomology, the application of entomological knowledge to the purely practical needs of man, entirely aside from those needs of the mind which satisfy themselves with the philosophical aspects of any problem. Its function is that of controlling those insects which in one way or another come into conflict with man and of utilizing those forms that may in one way or another be useful. Its immediate aims have nothing in common with those which direct the activities of workers who are interested in insects from a purely intellectual point of view. Nevertheless, the two branches come directly into contact and the work of the economic entomologist is in a very high degree dependent upon that of the systematist.

For one thing, the first step in the investigation of any insect because of economic reasons is that of properly identifying it and of developing the ability to separate it from related forms. Our various quarantines, for example, are directed against specific kinds of insects, and their proper administration depends upon the ability of some one to recognize those species. Again, in the application of control measures it is necessary to know with what we are dealing, for the methods applicable to one species may not be suitable for even closely related forms. A clear illustration of this is to be seen in the case of the mosquitoes. Out of the many different mosquitoes that are known but a few are of importance as the carriers of disease. The differences between these species are such that only an expert can recognize the various forms with certainty. Yet they differ so much in habits that control measures which are applicable to one would have no effect upon another. Discrimination among them is essential. The same principle holds with varying degrees of exactness in the case of every group of economic importance.

There has developed the method of "biological control" of insect pests. It would be beside the point to attempt a discussion here of the principles upon which this is based, but it is pertinent to indicate that it depends to a very considerable degree on the fact that certain kinds of predatory and parasitic insects feed upon certain other kinds and may be utilized to reduce their numbers. Many of our destructive insects are so simply because they have come to us from different parts of the world and in the transfer have left behind them the parasitic and predaceous species that served to keep them in check. The obvious thing would seem to be to introduce those enemies also into the new habitat, and in general this is what is involved in the application of biological control.

It is evident then that upon the question of the proper identification

of these insects pests depends much of the application of this method. If an insect came to us from Africa, it may be assumed that its enemies will be found there and not in Australia. The problem of determining where the insect came from depends upon the work of systematists. Indeed, were it not for systematic work the very fact of the transfer of insects from one part of the world to another could be established only as a matter of inference, for its establishment is dependent upon the recognition and discrimination among the different kinds.

One could go on indefinitely multiplying examples and building up the case for the systematist. These illustrations that have been given, however, will be sufficient to indicate the scope of his usefulness even in the most severely practical work. The fact that such work quickly passes from his hands does not detract at all from this usefulness and from the necessity of depending upon his special knowledge.

It is only fair to note that the systematist in turn has profited by the stimulus given to all entomological study through the development and increasing importance of the economic branch. This has come not merely through increased interest in insects in general but through the increased necessity for students of the various important groups. Even the "purest" of systematists, working perhaps on a group that does not have and possibly never will have the slightest economic importance, probably owes his opportunity thus to work largely to these factors.

It follows then that while the systematist has, on the one hand, entirely legitimate functions which are purely his own and are concerned with the development of the philosophical aspects of his study, he has also obligations to the more purely practical workers. And in turn the practical worker, with his own specialized and highly important aims, is indebted to the systematist in no small measure. The two are consequently to be regarded as parts of a co-ordinated whole, partners in a most genuine sense.

Because of this there devolves upon the systematist a certain duty. He "is not altogether a free agent; his work not something that concerns himself alone, or only those who may chance to share his interests. He is an essential co-worker in a common cause; his work a necessary part of a science of the greatest economic importance. . . . . This fact imposes certain moral obligations . . . ."[7] upon him. Remembering all this he should strive to do his work in such a way that it may attain its greatest usefulness and that he may discharge his entire obligation to his science and to his fellow-workers.

[7] Heinrich, Carl, *Proceedings, Entomological Society of Washington,* XXVI, No. 3, editorial page, unnumbered, 1924.

And so the student whose interest leads him toward the work of the systematist may feel that in entering upon such work he is undertaking something that is fully worthy of his best efforts. That such work has passed through a period of disrepute is clear. But there are indications that its period of exile from the high councils of biology is drawing to a close.

I have already quoted the words of Bateson, one of the prominent workers in the field of genetics. Let me quote again from Pearl, whose position in modern biology and whose right to speak with authority is unquestionable:

> It seems probable that we shall before long witness a return to a saner attitude than has prevailed in the last quarter of a century in regard to systematic zoölogy and botany; and in the training of our students . . . . give them a more adequate conception than they now get of the orderliness and the diversity which together characterize animate nature as a whole.[8]

In view of all these things it is worth while for systematists to consider carefully the bases and the principles of their work; to reflect upon the question whether or not they have adequately fulfilled the requirements of their position as members of a co-ordinated whole; to ponder the legitimate question whether they themselves have not been in part responsible, through their own failure to progress in their thought and their practice as other fields of biology have progressed, for the disrepute in which they have at times been held.

And so the succeeding chapters of this volume are intended to consider—as far as entomology is concerned—these questions.

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[8] Pearl, *op. cit.*, p. 584.

CHAPTER II

THE SCOPE OF SYSTEMATIC BIOLOGY

We have so far discussed the contribution of the systematist to biology without stopping to define or to explain what is included within his activities. In fact it might at first glance appear that any special effort to elucidate something which is palpably obvious and consequently is quite generally understood will be quite superfluous. Nevertheless, it is by no means certain that the systematist and his work are either so fully or so generally understood as to make all such discussion unnecessary. There is evidence that even those who are themselves systematists do not always have such an understanding and that among those who are not systematists such a condition is by no means uncommon.

The explanation of this situation will depend very much upon the point of view. The systematist, who is quite content with the existing accomplishment and the existing standards of his particular field, will doubtless say that if there be any such failure on the part of others properly to appreciate his work it is their fault and not his own. Perhaps in part this is true. It is certainly not the usual thing for workers in any special field to expend much time and thought in an attempt to appraise at its fullest value the efforts of those in some other. So it can hardly be expected that others will accord to the work of systematists a scope and importance greater than the systematists themselves grasp or attempt to express. Certainly if the defining of their activities be left to others it can not be expected that the resulting definition will be anything more than a limiting and a restricting one.

It is of course a difficult matter to keep always before one's eyes the broad conception of one's work, to remember always that the final aim is to be found in something more than the mere accomplishment of detail, to remember that the parts must be done merely that the whole may finally be developed. And so if systematists have all too frequently failed to grasp the full potentialities of their work and if they have all too frequently limited themselves to the attainment of objectives which are in themselves but means and not ends, such failure is but the expression of a very common deficiency in all forms of human endeavor.

It is possibly true that for all ordinary purposes the necessity of fixing one's eyes upon the immediate objective is predominant, but nevertheless it is well occasionally to look about and to reflect upon the broader implications of one's work, upon its place in the system of which it is but a part. This being the case let us so reflect a bit upon the broadest definition of the systematist's work, not upon the most narrow. It is

impossible that one who elects to become a systematist will ever utilize or even touch upon all of the possibilities that may lie within such a definition but at least he will not have made the mistake of limiting himself to a too narrow conception of what he may do.

First of all: What are the activities in which the systematist engages? Which of them are means; which are ends; what are their implications?

Obviously the very first thing which the systematist does is to attempt to discriminate among species. It is with species that his work begins. It is with their recognition, their classification, their relationships, that he is later to concern himself. This being so it may quite logically be asserted that the first of all problems with which the systematist is concerned is that of what species are. And the question of what species are is most intimately connected with a number of other questions, which together make up one of the primary fields of the study of life, that of the study of organic evolution. They are the questions of how species come into being, of how they are perpetuated as species, of their history, of their lines of descent.

Now, curiously enough, although this question of what species are, together with the other questions which are associated with it, is from a philosophical point of view the central thing with which the systematist is concerned, he has since the time of Darwin and Wallace to a large extent lost his grip upon it. It is this same question with which the student of experimental evolution is concerned; it is the same question with which the student of genetics must begin. It is this question with its attendant implications which constitutes what Bateson has called "the very salt of biology."

It is quite true that systematists still discuss these questions but it is equally true that of recent years their discussions have been but little heeded by the workers in these other fields. To quote again from Bateson:

> I had expected that genetics would provide at once a common ground for the systematist and the laboratory worker. This hope has been disappointed. Each still keeps apart. Systematic literature grows precisely as if the genetical discoveries had never been made and the geneticists withdraw each more and more into his special claim—a most lamentable result. Both are to blame.[1]

Let us note carefully the statement, "Both are to blame." With the portion of the blame which falls upon the geneticist or the student of experimental evolution let us not concern ourselves. Let us consider the part which is our own. The failure, fundamentally, on the part of the systematist lies in his refusal to make use of or even to learn anything

[1] Bateson, *op. cit.*, p. 60.

about the contributions made by the workers in these other branches, in fact in his frequent utter unawareness that such branches even exist.

If it be admitted that the problem of the nature of species is the central one of the systematist's endeavors, shall he permit the means for the solution of the problem to pass entirely from his hands into the hands of others? Were it not better to say that anything which has a bearing upon this problem is legitimately a part of his own field? Is it not in fact true that anything which will aid him in this portion of his work, anything which throws light upon his problems, is just as much a part of systematic work as are the conventional activities with which he has always contented himself?

It is the writer's contention that such is the case. It is his contention that the systematist in his attempts to deal with these problems is not merely justified in utilizing any means which may have been developed, but that if he desires to regain any of his grip upon these portions of his work he is in very fact compelled to do so. There is no valid reason why he should not say that he will add to his other activities the branch of experimental evolution in the form of "experimental systematic biology."

Now, although this problem of the nature of species and his possible contribution toward its solution constitutes the purely philosophical portion of the work of the systematist, he is by no means always and immediately concerned with it. There are also the purely practical aims of his work, the accomplishment of which consumes the bulk of his efforts. It will be well to consider these other things carefully in order to arrive, if we may, at some expression of their actual place in the system of his activities.

Owing to the very obvious fact that it is extremely difficult, if not for practical purposes impossible, to discuss any large number of things unless we have some sort of name or other symbol by which to refer to them, the mere naming of species has taken an extraordinarily large part of the time of systematists. Confronted as they have been during the last few centuries by the enormously expanding knowledge of the richness and diversity of life, it is not at all to be wondered at if they have at times been unduly impressed by the apparent desirability or even necessity of bringing to the attention of the world as many species as possible. Even today the feeling that there is a very high degree of value in such work, of and by itself, has by no means lost its hold.

That such activity has unquestionably had its value we have already pointed out. But that its value today is such as to justify its vigorous continuation as any major objective of our systematic work, as any end in itself, is seriously to be questioned. The writer would not for an instant be interpreted as saying that no more new species should be described.

Such a view would obviously be absurd, for in many cases it is only through the discovery of new forms that we are at all enabled to understand those which have already been named. Nor may it be denied that in this bringing to light of forms which no one has ever seen before there is a decided element of pleasure.

But this attempt merely to add to the number of named species has frequently been allowed to assume an entirely undue degree of importance. There have been all too many systematists to whom the mere naming of "one hundred new species of the Genus So-and-so, new to Science" has been the very ultimate of achievement. Merely to add hordes of new forms to the enormous mass of partially assimilated or even entirely undigested forms which now compose the body of our systematic literature is simply to add to existing difficulties. If the addition be part of a process of elucidating and improving upon existing information, it is of course justified; but too often it has nothing at all to do with any actual increase of knowledge. Too often it merely adds more unrecognizable species to the already long list of such encumbrances.

In connection with the matter of naming it has been necessary to develop rules of procedure concerning the method of giving names and to aid in determining what name shall apply in any particular case. Again, this is undoubtedly an extremely important part of the work of systematists. In fact it is so important that at times it has seemed almost to overshadow in the minds of many systematists the proper objectives of their work. Many systematists seem to have forgotten that after all names are merely symbols and that it is really the things for which the symbols stand in which we are interested. On the other hand, it may be argued with equal force that all too many systematists have forgotten the importance of the symbols and in thus forgetting have made it more difficult to deal properly with the things themselves. After all, most of our thinking is done in terms of symbols, and if the symbols be mistreated and confused our thinking also is likely to be confused.

These circumstances have led an entomologist to say: ". . . . there is such a thing as nomenclature, which is by way of being a science of symbols, and which because of our sinning with symbols has become also something of a nuisance."[2] The truth of his remark will appeal to anyone who has ever engaged in systematic work.

And so this also, although an extremely important portion of the systematist's work, is but a tool.

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[2] Heinrich, Carl, *Proceedings, Entomological Society of Washington,* XXVII, No. 7, editorial page, unnumbered, 1925.

Coincident with the matter of naming species it has always been considered necessary for the systematist to give some sort of a description of them. In fact it has been accepted as a general rule that no name is really valid until the object to which it applies has been in some way described. So closely have the two things been linked that they are frequently regarded as synonymous and to say that a species is named is to say that it is described; to say that it is described is to say that it is named. Or even a further step has been taken and the word "known" has been employed as a synonym of both these other terms.

As a matter of fact one cannot deal very long with any very extensive part of our systematic literature without coming to the conviction that actually this interchangeability of terms is merely a conventional fiction. Yet the fact that this fiction has existed is undeniable as is also the fact that it has been extremely harmful to the proper progress of systematic work. Through its working, the *describing* of species—whether they be named or not—has too frequently been subordinated to the *naming* of them. It follows that of our *named* species—in entomology at least—a very large proportion are actually not *described* at all, at least within any real and practical meaning of the word. Furthermore, of those which are described but a very small percentage can be regarded as *known,* within any such meaning.

It is a major thesis of this volume that the proper describing of species in order that they may really be known is the most important *practical* task which the systematist has to perform. Second only to the matter of determining what are and what are not species, it constitutes the basic portion of all systematic work and upon the way in which it is done depends the validity of everything which follows it.

But once more, important as this task of describing species may be and however large a part of the systematist's time and effort it may consume, it is still but a means to an end. In any well-considered statement of the function of systematic work it can be regarded like the other things which we have discussed, as a most important instrument only.

Our species having been recognized and named and described, the next step in the series is that of arranging them—or rather the symbols which represent them—into some sort of system which will display in the most advantageous and most nearly correct manner the facts that have been discovered. This is taxonomy. In general, when he has arrived at this stage the systematist has been content to consider his labors as finished.

Again we would maintain that he has succeeded merely in the elaboration of another tool and that this business of classification, however important it may be, is not an end in itself. Of what use is all this elabo-

rate classification of and by itself? Its value lies solely in the aid which it can give in the understanding and the interpreting of the facts which it reveals. Such facts have again to do with the problems of organic evolution, of the relationships, of the lines of descent, of the history in time and space of the organisms with which they deal and with the bearing of all these things upon the problems to which we have referred in our discussion of the contributions of systematists to biological thought.

So it follows that the final step of interpreting the facts which have been revealed through all these laborious processes is the ultimate function of the systematist's endeavors. And if he discard this last step as no portion of his work he is cutting himself off from meeting on any common ground with the workers in the other fields of biology. In fact the question may legitimately be raised whether he is not actually excluding himself from the right to be called a biologist at all.

So we come at last to the point where we may attempt to formulate our definition of the scope of systematic biology. Perhaps this will accomplish our purpose:

Systematic biology should include in proper proportion all those activities which arise from or are connected with the study of those aggregates of organisms which we call species. It is in its broad implications essentially synonymous with the study of organic evolution.

CHAPTER III

THE PRINCIPLES OF SYSTEMATIC ENTOMOLOGY

Out of all these activities which are a part of the work of the systematist there are two which are of primary importance. The first, of necessity, is that of *segregating* species, that is of determining which of the organisms with which the systematist deals are actually different kinds. The second, which for lack of any better expression we may call the *identification* of species, includes all the processes involved in making it possible to utilize the results of the first step.

Upon the way in which these two primary activities are accomplished depends the entire value of the work of the systematist, for they are nothing more than, first, the discovery and, second, the recording of the basic data. It is evident that if, in the first place, the data be erroneous, or if, in the second place, they be so inadequately recorded that they cannot later be used, the possibilities of any real contribution from our systematic work are vitiated. It is upon these steps that any conclusions of general philosophical significance and any of purely practical value as well are absolutely dependent. For if the segregation of species has been so badly done that the word "species" has no significance or if the recording of the data has been so badly done that those data cannot later be utilized, of what value are any conclusions that may be based upon such work?

It would seem then worth while to inquire into the way in which this work has been done. What are the principles that have governed it? By what standards has its degree of success been measured? If these principles be not clear or these standards be inadequate, what is there to offer in their place?

In the matter of the segregation of species there is involved an extensive series of problems that will demand a lengthy discussion. Such a discussion will appear later. As the greater part of our work is intimately connected with the process of identifying species, regardless of how they may originally have been segregated and regardless of the general problem of just what species are, it is here that we can best see the workings of whatever methods have been utilized and can best test their adequacy.

If one searches through some reasonably representative portion of the enormous body of our systematic literature in entomology with the desire to discover the standards that have governed this work, one is likely to conclude that no such standards have existed. It is a plain fact, from which there can be no escape, that a very large part of the work of systematic entomology—to say nothing of other fields—has been very

badly done. Lest it be thought that this somewhat pessimistic view is peculiar to the writer, let us note the remarks of McLeod concerning this.

> The systematists confined in an antiquated routine, are the first victims of their deplorable method. Since the great majority of the existing specific and even generic descriptions are superficial and unfinished to such a degree that the exact identification of a specimen is simply impossible, the systematists *discover* continually and describe under new names so-called *new* species, which have already been described either once or even several times. The result is a complicated synonymy and a discouraging disorder.[1]

And further

> There has been a breach of continuity in the historical development of Biological Science. The classical work of Linnaeus, the De Jussieus, Lamarck, Latreille, R. Brown, the De Candolles, Deshayes, R. Berkeley, Reichenbach, Von Martius, W. and J. D. Hooker, and other masters of descriptive science, who tried to inventory and to classify the specific forms, has made hardly any *substantial* progress for half a century. It has been, as it were, interrupted when the doctrine of the constancy of species was replaced by the idea that specific forms are transformed one into another by the accumulation of impalpable changes. It is true many new species (or so-called new species) have been discovered and described, but the new material has been studied almost entirely according to old-fashioned methods. The specific and generic descriptions . . . . are on the whole drawn up in the same way as a half-a-century or even a century ago. . . . . As far as the method is concerned several of the new works (in Botany) are rather inferior.
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> On the other hand scientific research along new lines has obtained more prominent positions: much progress has been achieved in anatomy, cytology, embryology, physiology, hybridization, heredity, variation, biology properly so called—that is to say, in those parts of zoölogy and botany which might be called *laboratory science.*

It is possible that this statement is somewhat exaggerated, and yet it contains a great amount of truth. The plain facts are that our systematic work has not progressed as it should have done; it has not kept pace with the development of other branches.

This is not to say that all systematic work has been badly done. There has been much which has been as well done as the limitations of the times, the material and the methods of work would permit. There has been much that has been genuinely masterly, bearing comparison with the scientific work of any field. But unfortunately, if we balance it all up, taking into consideration the real aims of systematic work, it appears that probably the greater part of the contents of the thousands of volumes

[1] McLeod, Julius, "The Quantitative Method in Biology," *Publications of the University of Manchester, Biological Series,* No. 2, page 201, (1919).

that contain the record of the work of systematists is in a greater or less degree inadequate, even making the utmost of allowance for every extenuating circumstance.

The reasons for this condition are many and various. Some of them are common to all forms of human endeavor. Some are peculiar to the general field of systematic biology, and some to the special field of systematic entomology.

Knowledge does not usually spring into being fully developed and ready to bear fruit. It is a slow and struggling growth, and, like the mighty oak, the center of the trunk which was once the young and growing tree may in time come to be nothing but a hollow cavity. So to recognize the fact that the work of the earlier authors is inadequate when judged by the standards appropriate to the present day is merely to accept a general truth. The fact that probably not one of Linnaeus's descriptions in and of itself will permit the definite recognition of the species which it purports to describe, does not at all detract from the value of his work. The fact that the first locomotive is now a museum piece, which compared with a modern locomotive is somewhat amusing, does not at all detract from the credit due its inventor. It is the inevitable result of progress that the thing which was once the very ultimate of achievement is now left behind and of value only as whatever of good it possessed is now incorporated into the general body of knowledge.

To call attention to these changes, then, is in no way to disparage or to criticize the men who laid the foundations upon which our present work is based. But to continue to accept their standards is to deny the necessity of progress. We cannot today utilize their work and their methods any more effectively than the railroad of today can utilize one of the earlier locomotives. In fact, as McLeod has rightly and clearly pointed out, some of our difficulties are to be ascribed to the fact that we do actually employ in our present work the unimproved tools of a hundred years or more ago. Descriptions can be found in entomological journals of the last year which are in no way an improvement upon the work of Linnaeus of over a hundred and fifty years ago.

There are also historical reasons having to do with entomology as a special field which account in part for certain of its deficiencies at the same time that they account also for some of its development. It should be borne in mind that entomology as a profession, as a branch of biology in which and by means of which it is possible to gain one's livelihood, is a comparatively recent development. In its earlier days it was almost purely an avocation, a hobby, engaged in by those who—at least in the opinion of their neighbors—were mentally somewhat peculiarly constituted.

And even today, when it has become a profession, respectable and respected by all who know anything of its activities, it still partakes of the characteristics of an avocation, even with many to whom it is also a vocation.

This circumstance, I say, is undoubtedly responsible both for some of the defects of our systematic work in entomology, and for some of our progress. Had it not been for those who in its earlier days carried on the work without any thought of reward and often at much expense to themselves, we should still know little about insects. And the same holds true, but to a lessened extent, today, for were it not that there are still such individuals, as well as professional entomologists who give back to their profession in the form of financial support for entomological journals and the like a portion of what they earn from it, progress would be much slower than it is.

On the other hand, there have been certain unfortunate results from this otherwise admirable system. The persistence of the amateur—a thing in itself highly desirable—has encouraged the persistence of the *untrained* worker. The writer would not for an instant be understood to condemn amateurs as a class, for to do so would be to condemn some of the most able students that systematic entomology has had. But it is unfortunately true that the amateur, however admirable his spirit, has too often been entirely unfitted to undertake work that could be dignified by the name of "Science."

It is also unfortunately true that the standards of the untrained dilettante are still in large measure predominant in much of our systematic work. The standards of those who collect butterflies and beetles in the same way and with the same aims as others collect postage stamps are not and never can be the standards of genuinely scientific work, but their influence upon our activities, upon our entomological journals and upon our entomological text books is still clearly to be seen.

Certainly it would seem that a field of science into which an absolutely untrained individual can enter without giving the slightest evidence of his qualifications cannot be possessed of exceptionally high standards. Nevertheless, this admission of the untrained worker is a curious phenomenon that affects all branches of systematic biology alike. Other fields of science, such as chemistry, medicine, bacteriology, have largely escaped from it. In the systematic branches it acts with destructive force and in few more so than in entomology.

The persistence of this phenomenon is connected with the fact that systematic biology throughout comes as near to being a perfect democracy as anything well can. Within its boundaries all men are equal, not only in

theory—which might do no harm—but in actual practice. The work of any one systematist must receive the same consideration as that of any other. Under the existing canons the mentally unbalanced, the merely ignorant, the well-meaning but incompetent, the stupid, the trained, the intelligent, and the competent are all upon an absolute parity. To quote the very apt remarks of the forward-looking editor of one of our leading entomological journals:

> Under the present codes any ignoramus can write a few words about an insect, propose a new name—almost any kind of new name—for it, print his verbiage in any old way so long as he offers the product "for sale," and Entomology must take the burden of his "new species," the honest systematist worry with his unusable name.[2]

Furthermore, in addition to this democracy among contemporaries, there is a similar democracy of time. The systematist of today must still puzzle over the inadequate descriptions of a hundred and fifty years ago with the same assiduity that he is expected to give to the work of the present day; indeed, he is even expected to give more care to such work, if that be possible, in order that no particle of injustice may be done the memory of these earlier workers. He must still attempt to drive the "Puffing Billy" in the train that demands a modern locomotive!

All of which—if one reflect upon it—is an amazing situation, a situation that is responsible for a tremendous dead weight that all systematic work carries about its neck like a millstone. It is individualism run riot, acknowledging no authority and bowing to no law, not even at times to that very minimum prescribed by the various nomenclatorial codes. It is in fact almost the perfect expression of philosophical anarchy!

And yet we are confronted with the danger that even if it were possible to substitute for this condition some form of effective control looking toward the bettering of conditions, such control might develop into a tyranny. With all of its acknowledged defects, if individualism is to live anywhere, it should be within the protection of Science, which after all is its offspring. To sacrifice everything in the name of Efficiency might lead to other conditions which were worse. Various suggestions have at times been made for remedying these conditions, at least as far as they concern contemporary work; but the problem remains to be solved.

Another factor which reacts unfavorably upon the character of our systematic work in entomology is to be found in the vast extent of the field. No one man can command a critical knowledge of more than a

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[2] Heinrich, Carl, *Proceedings, Entomological Society of Washington,* XXVII, No. 2, editorial page, unnumbered, 1925.

very small portion of it. There must of necessity be a high degree of division of labor. So it follows that there are groups of insects of which scarcely more than one or two men in all the world know anything at all. Some of these groups are by no means small in point of numbers of species. The result is that in these groups there can be little or no critical comparison of work. If but one man knows anything of a group, who is to say whether his work be good or bad? From whence, other than his own inner resources, is any stimulus to better work to come?

And so it is not surprising that an examination of our systematic literature will reveal but little of definite common standards. The body of our systematic work constitutes a gigantic and in its details a badly built structure. Its larger outlines are fairly well defined, although even here there is confusion, as witness the varying numbers of orders of insects recognized by different workers. As we approach the smaller details we find all too often that much of the work is practically useless. In some groups where masters have labored the work has been well and faithfully done. It is evident that in some cases the workmen have labored with honesty and intelligence and skill. It is equally obvious that others have been ignorant blunderers, failing even to make the best of the tools and material that were available. It appears even that some have been dishonest, striving merely by meretricious activity to attach their marks to as much as possible of the structure.

It is consequently a structure in which unity of design and execution are largely lacking. There has been work of the highest quality and work that by no permissible distortion of the meaning of the word can be called Science. The best has been good, the average has been bad, the worst has been worthless or even actually harmful.

In addition to these various circumstances which have been considered, there is the further fact that even competent systematists seem to have failed to agree upon any common standards in their descriptive work. To describe a species would at first glance appear to be a simple thing, and yet if one reflect upon it the problem is not so simple after all. Granted that we have before us all the pertinent information from which our description is to be drawn, the problem arises as to how far we shall go in our description. To describe in detail every structure that is to be seen even upon a simple organism is an impractical thing. To place every hair in relation to every other hair, to describe every joint, every furrow, and every ridge, to locate every punctation and every rugosity, to define every curve, is for all practical purposes an impossibility, one may say. And yet, without doing all this, so to describe the organism that others may be able to recognize it definitely, that the labor which has been ex-

pended in segregating it from its fellows and the facts gained in the course of this labor shall not be lost, is a difficult thing. It is not at all astonishing that enormous disparities appear even in the work of really competent students, that almost no two solve the problem in exactly the same way.

Consequently the attempted solutions of the problem have been many and various. Some workers in fact have apparently never recognized that the difficulty exists and have perverted the description, making of it merely a tool for the purpose of "establishing the name," which, as we have pointed out, is no legitimate part of its function. Others seem to have given up the task as hopeless and have made the description a purely perfunctory thing, placing their reliance for future identification of the species entirely upon the types; others have honestly recognized the problem and have essayed its solution, with varying degrees of success.

The writer, when he was a student, approached one of his instructors with this problem. The reply to his question as to what should be included in a specific description, was substantially this: "The general practice is to give enough to permit the separation of the species from the others of its group that are already known." It of course cannot fairly be said that this standard has been adopted by all systematists who have attempted to answer the same question. Even so, if one search through the literature it will appear that such an answer is about as near as one can get to any general expression of the common standards. Probably very few entomologists have limited themselves strictly to it—a few have not even attained it—for its strict application would result in descriptions such as even today are being published in one branch of entomology and of which the following is an actual example, taken from a journal of the year 1925:

> As *nigra* but legs as in *io,* abdomen 5 more than twice 4, latter a bit shorter than 2. Post marginal distinctly shorter than marginal, exceeding stigmal. Clothing inconspicuous.

Nevertheless, the influence of this standard is clearly to be traced throughout the greater part of our systematic work.

That such a standard should quite generally be accepted is easy to understand. Confronted by an organism belonging to a group that contains an entirely unknown number of species still to be discovered and these consequently possessed of an entirely unguessable variety of form, with the practical impossibility of describing in words absolutely everything that is to be seen upon the organism, it would appear to be a practical, if not a logical, solution.

From this circumstance, doubtless, has developed the custom of giving a diagnosis. That is, the observer selects from the characters which he

sees those which by actual comparison with other species appear to him to be important, and records them briefly. In addition to those so selected, he may note others which will possibly be of use when more species are known. He frankly discards all thought of anything approaching a "complete" description.

It is the writer's contention that no genuinely scientific system can be evolved on the basis of such a standard and that as long as it prevails our systematic work will fail to attain the degree of effectiveness that may reasonably be demanded of it.

In the first place, it is evident that such a standard, or any modification of it, rests upon an almost purely subjective basis. It depends entirely upon the judgment of the observer. Considered in connection with what has been said as to the absence of any standards governing the qualifications of that individual it can be seen that even if the standard were logically valid its application is likely to lead to but deficient results. It is evident that while some workers may possess a keen and discerning eye and excellent judgment, others will not. It is quite true that there have been systematists with an almost uncanny ability to select those features which are really significant, but such individuals are rare and even they, by the very nature of things, cannot but fail in many cases. As for those of lesser ability and insight, the results have many times been a mere waste of words and printer's ink.

But even if all systematists were possessed of the most excellent judgment, the system would still fail in part, for it is fundamentally illogical. There is nothing in such a system that is fixed and permanent. The data which are presented are purely relative. The species are described with reference to each other, not with reference to any fixed "datum plane." Under such a system, evidently, our points of reference and of comparison shift constantly as our knowledge changes.

We have, let us say, two species, one black and one white. They can easily be separated on this basis and we need look no further for differences. But suppose there are found several more species, some black and some white, and differing from each other in structural characters also. How is one to separate any of them from the first two, of which the structural characters are not known? It is evidently necessary to go back, to re-examine these first two forms and "revise the genus." As our knowledge grows this process must be repeated many times. This is in fact a common procedure, necessitated by the acceptance of such a standard.

This might not be so unfortunate if the original specimens were always carefully preserved and always easily accessible, but such is not the case in practical work, under conditions which bring it about that the workers

upon any particular group may have half the world and many years of time between them.

As a concrete example of this, we may take that cited by McLeod on page 66 of the work already quoted.

> We find in a classic book, which contains the descriptions of about thirty-five species of the genus *Carabus,* that in one of these species the ultimate joint of the maxillary palp is shorter than the penultimate. For all the other species no information is given about the relative length of the joints mentioned. We are also told that the ultimate joint of the maxillary and labial palps is broad in certain species and narrow in others, but for the majority of species this characteristic is passed over in silence.

His experience is merely that of every systematist seeking to derive from the literature an understanding of any particular group of species, especially where that group has been dealt with by different authors and at different periods. It cannot be otherwise under such a system as that which has prevailed.

The system, then, has two most serious defects: that it can never be anything more than subjective, dependent upon the abilities, the judgment, and the equipment of those who apply it; and that it can never be anything more than partial and relative. Whatever of enduring worth may come from the application of such a system will arise from the fortunate combination of chance and the ability of workers who are superior to the system; it will come in spite of the system and not because of it.

Probably the majority of modern systematic workers in entomology have in some degree or other recognized the deficiencies of the existing standard and have attempted to escape from them. The extent to which these deficiencies will be realized and admitted varies. Some, who are of a patient and forgiving nature—or is it that they are blind to the actual conditions?—will hold the deficiencies lightly or will even deprecate the use of the word "failure" in referring to them,[3] and will excuse in one way or another the conditions which force them to pore over inadequate desciptions in the search for some clue to the identity of species, or to send specimens half way around the world in order that they may be compared with types, or to scheme and plan and save in order to secure the funds with which to visit distant museums for the purpose of examining inadequately described material; conditions that force them to take into consideration every possible shred of evidence that may be available, down even to the handwriting on pin labels, in order to arrive at as much as a guess at an identification.

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[3] Rohwer, S. A., *Journal, Washington Academy of Science,* XVI, 53, (1926).

Others will occasionally protest and urge their colleagues to greater care and definiteness.[4] Some will urge that descriptions be made longer, and others will say, "Let us have fewer words and more figures. . . . . A drawing of structural details—even though it be crude and esthetically unsatisfying—is more enlightening than a torrent of the most carefully selected adjectives."[5]

Some will protest against long descriptions, claiming that they serve merely to confuse the user, and will urge as a substitute greater clarity of expression and more careful study to determine what is really important. Some will advocate the adoption by entomological journals of certain minimum standards to which papers must conform before being admitted to their pages; at least one journal has in fact adopted such rules in order to improve the standard of work. McLeod, to whose book we have several times referred, has proposed as a remedy the adoption of a quantitative basis for specific descriptions.

Yet out of all this there comes very little in the way of a definite program or a definite statement of standards. All of these things are merely palliatives, attempts to patch up a structure which is inherently faulty. What is needed is a complete restatement of our aims and a clear formulation of the principles which should govern our work. It may be admitted to begin with that any ideal which may be expressed must be modified to conform to practical requirements, yet it appears possible to devise a series of statements that shall be at least theoretically sound, regardless of how widely opinion may vary as to their practicality.

And so the writer presents the following theses which he would defend as conforming to the theoretical requirements. It is his conviction also that they admit as well of an adequate degree of attainment in practice.

I.

The segregation of species and their minor divisions is the first step in all systematic work, and for its accomplishment the systematist may and should employ any means that are available in order to arrive at a knowledge of the biological facts, whether those means be found in morphological, anatomical, physiological, experimental, genetical, or even chemical studies.

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[4] Muttkowski, R. A., "The Composition of Taxonomic Papers," *Annals, Entomological Society of America,* IV, 194–217, (1911).

[5] Heinrich, Carl, *Proceedings, Entomological Society of Washington,* XXVI, No. 6, editorial page, unnumbered, 1924.

II.

Owing to practical considerations, as well as to the facts of biology, the greater part of our work in systematic entomology must be based upon the study of external structures, that is, upon morphology. It follows that morphology and systematics are essentially synonymous and inseparable. A genuinely scientific system demands that every structure which is to be found upon the body of the insect be taken into consideration, and until that has been done our work is to be regarded as incomplete.

III.

If a scientific method demands that every structure be taken into consideration, it follows as a necessary corollary that the material with which we work must be of such a nature as to permit the realization of this ideal. The development of the necessary technique in the preparation of material is a part of the requirements of systematic work.

IV.

The second step in systematic work is the recording of the data obtained in the course of the first in order to make possible its future use for the purposes of identification. In order to obtain a fixed basis, a fixed datum plane, it is necesary that all the data be recorded and that a technique be developed which will make this possible.

V.

The mere recording of the data, however well it may be done, does not conclude the work of the systematist. It is necessary also that these data be analyzed and their relative importance and significance determined as far as may be in order to facilitate their future use. This is the principle function of the verbal description.

VI.

The data accumulated in the process of segregating and describing species are the basis for the development of a classificatory system. It is the aim of such a system to chart the course of evolution, to express the varying degrees of relationship of the organisms involved. The essence of such a system is the arrangement of the organisms into natural groups of varying rank. The controlling standard is that of revealing in the clearest and most practical way the conclusions that have been derived from the preceding steps.

VII.

As a part of the practical work of the systematist the utilization of a system of nomenclature is essential and this system, in order to be generally usable, must be governed by a system of rules by which systematists everywhere and always should abide.

VIII.

The doing of all these things which are involved in the work of the systematist is a technical task which should be undertaken only by those who are technically trained for its accomplishment. The acquirement of this technical training should be considered essential for anyone who wishes to be called a systematist.

CHAPTER IV

THE SEGREGATION OF SPECIES

The segregation of species and their minor divisions is the first step in all systematic work, and for its accomplishment the systematist may and should employ any means that are available in order to arrive at a knowledge of the biological facts, whether those means be found in morphological, anatomical, physiological, experimental, genetical, or even chemical studies.

It would seem that all of this is self-evident, for it is with the recognition of species and discrimination among them that the work of the systematist begins. It is from species that his classification is built up. It is from the facts revealed by such work that his conclusions of general biological significance are drawn. It would appear then that toward the solution or at least the clarifying of the problems associated with the nature of species there should be employed every means that may possibly be of aid.

Yet we are confronted by the curious fact that systematists in general have allowed the consideration of these problems to pass very generally into other hands. They have in fact quite consistently excluded from the range of their activities all those things which the students of other branches have done in their attempts to arrive at some answer to these same problems. Let us revert to the remark of Bateson, already quoted: "Systematic literature grows precisely as if the genetical discoveries had never been made."

As a matter of fact there are two kinds of "species." There are those groups of individuals which the systematist, working as he must merely from preserved specimens—if he be content to employ only the conventional methods—labels by a common name. And there is that entity or concept—whichever it may be—that the student of evolutionary processes calls a species. That these two things are by no means necessarily synonymous does not perhaps occur to most systematists, but undoubtedly the geneticist or the student of experimental evolution would recognize the distinction. From the point of view of these last-named workers any consideration of the question from the standpoint of one who deals exclusively with "species" of the first sort is simply wasted time. I do not question at all that in this very circumstance is to be found much of the difference of opinion as to the value of systematic work which undoubtedly exists.

The plain facts of the matter are that the systematist working merely

from preserved specimens has but little real contribution to make to the theoretical questions as to what species are and what are species. The very best that he can do is to arrive at some purely provisional arrangement, a preliminary and extremely rough sorting which will serve as a starting-point for further and more critical study by more refined means. And the worst that he can do—and too often has done—is simply to make a hopeless jumble of the whole business.

We may quote here from a volume by Hall and Clements[1] to which we shall recur several times in the course of this discussion and which we would urge anyone who is interested in the general problems of systematic work to read with care. The fact that the volume is concerned specifically with botany does not in any way change the applicability of its point of view to entomology or any other field. With everything that is said in this volume one need not necessarily, and probably will not, agree, but it remains an exceedingly stimulating contribution.

The authors remark (page 9): "If only the end results of this process [of evolution] are known, as in the case of herbarium studies, they appear to be distinct units, often with little or no evidence of their common origin. As a consequence units of every possible degree of differentiation and value are masquerading as species in existing taxonomy." The truth of this statement is undeniable.

Yet there are certain practical considerations which make it impossible to escape entirely, or even to any very considerable extent, from this condition. For one thing, it is usually not possible to bring into a collection any very large representation of a species. While perhaps even five hundred or a thousand specimens are all too few to represent adequately the range of variation of species which are represented in the field by millions of individuals, it is utterly impossible in the majority of cases to house any such number, even if they could be obtained. In the majority of cases they cannot be secured. The systematist working from preserved collections must simply make the best of what he can get. Nearly every known organism has originally been described from at the most but a very small number of specimens. It is probable that the greater part of all the organisms which may be described in the future will likewise be based upon such scanty material.

This, then, is a situation from which there is little opportunity of escape. It is an inevitable accompaniment of the systematist's work and the only valid criticism that can be voiced of him for accepting it comes

[1] Hall, Harvey M., and Clements, Frederic E., *The Phylogenetic Method in Taxonomy*, Carnegie Institution of Washington, Publication No. 326, (1923).

when he so far forgets the limitations of this method as to think that the "species" which he names in this way are anything more than merely provisional arrangements. They are little more than names attached to a larger or smaller number of specimens in a collection serving roughly to index them and to facilitate further treatment of them.

There is also room for criticism when the systematist becomes so absorbed in the work of describing and naming species of this sort that he entirely forgets the species of the other kind, those species which represent the actual basis and the only valid basis for any kind of theorizing and for the formulation of any conclusions as to the problems of evolution. In actual practice the two things are quite distinct, but it should be one of the ambitions of the systematist to bring as many of the two sorts of species to a common ground as he may.

It necessarily follows that the criteria for the segregation of species under these different conditions must be different. The systematist confronted by the necessity of discriminating as best he may on the basis of his preserved specimens will find it necessary to employ certain largely empirical and arbitrary criteria, at least in a very large proportion of the cases with which he must deal. In perhaps the bulk of his work these criteria will for all practical purposes be quite adequate, at least for the purposes of a tentative and provisional arrangement such as the greater part of our systematic work must be. But they will not be adequate and must be superseded by others as soon as the attempt to bring the practical and the theoretical aspects of his work into harmony begins.

It may very well appear at first glance that the practical answer to the question of what constitutes a species would be a simple thing. Species are kinds of organisms, and consequently different kinds of things are different species. But actually the problem is by no means so simple. It appears that the question of what constitutes a species is really a very complex thing. It is in fact a question that even yet has received no very satisfactory answer, or at least there is no very general agreement upon such an answer. There are a number of disturbing factors which enter in and complicate the problem.

For one thing, it is almost an axiom of biology that no two individuals, even, are exactly alike. The fact of individual variation is really at the base of much of our biological speculation; it is indissolubly bound up with every attempt to account for the origin of species. This being the case, how far may individual variation go before the varying individuals become different kinds of things? We meet with some most extraordinary complications the moment that we begin to inquire into this question. Consider the case of the domestic dog. There are perhaps a hundred distinct and

quite easily recognizable races, ranging in form from the mastiff or Great Dane to such miserable travesties upon the name of dog as the hairless Chihuahua. There are an infinite number of combinations of these various types and there are also an infinite number of combinations that are unclassified and unclassifiable. Yet all of these forms are considered to be a single species.

In like manner the various races of man, together with all their now hopelessly entangled crosses and individual variants are considered to be but a single species. Even within the limits of species under natural conditions there may be a bewildering range of variation in form, size, and color. On the other hand, there are thousands and thousands of kinds of organisms separated from each other by characters so small that, compared with the differences among the various races of man or of domestic dogs, they appear absurd.

How are we to reconcile these discrepancies?

In part they cannot be reconciled. They are simply an expression of the fact to which we have already referred, namely, that under the name of species there masquerade forms representing a wide range of value and significance. In part these discrepancies are to be accounted for by the different criteria that have been applied to these forms.

For example: If we knew the domestic dog solely from a single representative of each of the various races we would almost certainly call each of these races a distinct species. We should then be in exactly the situation in which the systematist working from preserved material usually finds himself. Under those conditions such action would not only be justifiable, it would be almost unavoidable. But in the case of man and of the domestic dog there is available much additional information which permits us to come to other conclusions. The nature of that information we shall discuss later, but we can plainly see that the discrepancy arises from the application of two different sets of criteria.

If we confine ourselves to preserved specimens alone there is but one actual criterion of species that we may use in all cases and regardless of the amount of material available. That is the criterion of degree of difference, of recognizability. As Hall and Clements remark (page 10), "the working definition of the average taxonomist is distinctness, by which he understands the ready recognition of a particular form." This is an extremely poor criterion and it may lead to conclusions which from the point of view gained by the application of other methods are obviously wrong. Nevertheless, in the majority of cases it is all that is available with which to work. It is subject to all sorts of abuses and to all sorts of modifications, depending upon the nature of the material, the acuteness

of the observer, the equipment with which he works, his prejudices and his personal opinions.

Furthermore, because it is usually based upon an infinitesimally small representation of the total population from which the specimens may be drawn and because it can take into consideration but a minute fragment of the entire range of variation in the species which it represents, it is likely at times even to be misleading. Two specimens which taken by themselves appear to be quite distinct may upon comparison with a long series prove to be merely minor variations. Nor does it aid us greatly in the case of species which are strongly dimorphic or polymorphic. Among the insects there are many species which differ so greatly sexually that the two sexes cannot be correlated on the basis of specimens alone. There are also forms which are seasonally or otherwise dimorphic or even polymorphic, the forms differing so greatly that there is little or nothing in the specimens which will permit any judgment as to their connection with each other.

Nevertheless, with all its deficiencies this criterion of degree of difference, of recognizability, is, as we have said, in many cases absolutely the only criterion that is available. Consequently its employment may not be avoided. Furthermore, it may be pointed out that for practical purposes species must be recognizable. If they be not recognizable there is little gained in segregating them as species.

This criterion may be supplemented by another, if the amount and character of the available material permits. That is the criterion of lack of intergradation. Upon this criterion forms may be separated from each other, regardless of the degree of difference, if only that difference persist through a series of specimens. McAtee remarks,

> Even minute differences known to be non-intergrading are satisfactory for specific diagnosis, while characters apparently more important which vary so that intergradation is evident can be used only for the differentiation of categories of lower than specific rank.[2]

Again this criterion has such weaknesses that the results obtained by its application can be nothing more than a rough approximation, the expression of a theory of probabilities in the majority of cases. If we refer again to the case of the domestic dog and speculate upon what would happen if the systematist had available even a series of dozens or hundreds of specimens of each form from the poodle to the mastiff, taken, let us say, from the kennels of fanciers of each breed, we can see clearly the

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[2] McAtee, W. L., "Specific, Subspecific, and Varietal Categories of Insects and the Naming of Them," *Entomological News*, XXXI, 46–55 and 61–65, (1920).

results. Certainly the characters which define these various forms would appear constant and non-intergrading and on the basis of these specimens the systematist would be entirely justified in naming each as a species.

Evidently the same thing can occur in the case of specimens collected under natural conditions. A series of individuals collected in a restricted locality—say from a single tree—might very well resemble each other more closely than they would a series taken a relatively short distance away and yet a sufficiently long series of specimens taken from a sufficiently large number of localities might show the differences to intergrade.

Again there is the possibility of difficulty in the application of this criterion because of differences of opinion as to what constitutes intergradation. Let us take a hypothetical case in order to make this clear. Suppose we have two apparent species distinguished from each other by the fact that one possesses ten of a certain structure and the other has one. Is then a specimen with five of these structures an intergrade or a different species? And what are we to say of specimens with four or six, three or seven, two or eight or nine of the structures? One might argue—on the basis of specimens alone—either that each of these forms is a species or that all are variations of a single species and actually, still on the basis of the specimens alone, know no more of the real truth. Of course, there are cases where the examination of enough material will make the facts evident, but there are also cases where the more material that is examined the more difficult becomes the problem and the more incapable of solution by such methods.

That this last difficulty should logically arise if our theories of evolution be true expressions of biological fact is evident. The Darwinian theory of evolution demands that species shall arise by selection among a series of small variations. Even the mutation theory of their origin will admit that the variations, while "discontinuous," may be extremely small. Consequently, theoretically at least, if we had available an absolutely complete series of the stages through which all existing species have passed in the course of their development we should have a series of intergrades such that any separation of species which we might attempt would be largely arbitrary! The difficulty is by no means purely hypothetical, for almost any systematist can recall examples where his task would have been much simplified if he had had less material. In fact the history of systematic work is not devoid of instances where workers have been accused of throwing away the intergrades!

These two criteria, then, weak as they may be, constitute all that the systematist working purely from preserved specimens without regard to any other data, has available. By the judicious combination of the two, by

the accumulation of adequate material and by the most thorough study of what is available in order to make certain that all of the facts which can be obtained from it are known and have been weighed, the systematist has done all that he can by these means alone.

In addition to the actual specimens before him the systematist frequently—but by no means as often as he would wish—has available certain collateral information concerning the specimens. This may consist of the data as to locality, season of the year, ecological relationships, and other environmental factors. Usually, however, these data are but the bare minimum of date and general locality. But on the basis of these data systematists are frequently decidedly influenced in their decisions. Obviously if two slightly different forms are from localities hundreds of miles apart or separated from each other by some barrier through which they presumably might not pass it would seem more reasonable to regard them as different things than it would if they were taken in the same immediate locality. Yet such reasoning has little to support it and is just as likely to be wrong, when the species has been studied on the basis of more extensive material, as it is to be right.

The same sort of reasoning has been applied to other data of an ecological nature. In many plant-feeding insects and in many parasitic forms the matter of the host has frequently influenced the decision as to specific identity. Among the scale insects and the aphids such differences in host, especially if accompanied by any difference in locality, have at times had almost a predominating influence. Unfortunately such information, taken entirely by itself and given undue weight apart from or even in preference to the information to be obtained from the specimens themselves, has more often than not been misleading. But that it can be of value seems clear, and we shall revert to this point later.

In addition to these actual practical means of arriving at decisions concerning species, systematists in general have subscribed with more or less faith to certain theoretical conceptions, and these conceptions have sometimes actually been applied.

A very old theoretical conception of the species is that of non-fertility with other species, it being assumed that even if mating between distinct species takes place there will be no offspring or the offspring will be partially or wholly sterile. That there is some measure of truth in this belief, regardless of how valueless it may at times be considered, appears to be admissible. In the case of insects, for example, it may have a certain specialized application. In many insects the parts of the genitalia, particularly of the male, may differ so widely even in closely related forms as to indicate the probability that mating of a male of one species with a female

of another would be physically impossible. Under such conditions it would appear quite reasonable to conclude that if the two cannot possibly mate they may fairly be considered as distinct species, regardless of how closely they may resemble each other in other respects.

The conception undoubtedly also has been applied in the case of such animals as the dogs, the fact of their perfect fertility one with another being regarded as evidence that they constitute a single species in spite of their wide divergence in form. Also in the famous case of the species of *Oenothera,* upon which de Vries based his conclusions concerning the importance of mutations in evolution, the fact of partial sterility in the offspring has been utilized as evidence that these "mutations" are merely recombinations due to hybridization between distinct species. On the other hand, many plants, at least, hybridize so freely that this method of crossing is a standard method of securing new forms. So clearly is this recognized that certain botanists have even gone so far as to propose the theory that it is through hybridization that new species always arise!

It would appear from all this that properly interpreted this conception may at times have some value, but it has long been given up as having any very extensive application.

Another theoretical criterion has been that of constancy from one generation to another. Discussing this criterion Hall and Clements (page 10) say:

> A more generally accepted basis for the species is its constancy, or the ability to breed true. While it is doubtless true that practically all species do this many of the segregates of today [in Botany] do not. More significant still is the fact that the adherents of this definition make no attempt to determine this point before describing a new species. Constancy is undoubtedly a factor to be regularly taken into account in the evolutionary study of species, but it is futile to attempt this without the aid of statistical and experimental methods. On the other hand, the test of constancy alone would require the acceptance of an absurd number of species. For example Jordan (1873) recognized 200 species of *Draba verna* . . . . and the United States Department of Agriculture recognizes 250 varieties of wheat, all of which breed true and would thus come to be species.

On this same basis the various breeds of dogs would be recognized as species also, as would the various races of man. But the fact that they are completely fertile one with another, combined with the fact that consequently all sorts of mixtures and intergradations may be found compels their recognition as single species.

In actual practice this criterion is probably applied but rarely. In a way it is applied if there are available specimens representing different generations, even if these have not been reared one from another in controlled

experiments. For example, if numerous specimens of some supposed species be taken in different years and their characters are found to be constant from year to year it may reasonably be assumed that they breed true.

It is evident, however, that, aside from any theoretical assumptions which may be in the mind of the describer, in practice almost all of our named species have been based solely upon the evidence to be obtained from the specimens themselves and from the scanty collateral data that accompany them. In view then of the deficiencies which we have pointed out it is patent that this systematic work is at the best merely a series of approximations. In some cases it may chance that the approximations are quite close. In others, owing not only to the inadequacies of the methods employed but also to the ineptness of the systematist himself and his failure to make the most of such data as he has, they will be extremely rough. If the work of the systematist has been well done—as well done as conceivably may be within the limitations of the methods available—the results obtained will be adequate enough for most purposes in probably the majority of cases. But if any really precise information as to the status of a form is desired they can serve as nothing more than starting-points for further study by other methods.

In this connection we may refer to a pertinent statement by McAtee:

> In mathematics, the most exact of the so-called exact sciences, approximations are freely used, and some of them are among the most valuable possessions of the science. Surely taxonomists working in one of the most inexact of all fields of science should be satisfied with approximations, and what is more to the point, recognize that they are only approximations, and not to be taken as immutable entities, nor fit subject for pedantic positiveness, when in both respects they are the opposite.[3]

With but one of these statements would the writer disagree and that is the statement that that the taxonomist should be *satisfied* with approximations. While he must indeed work with approximations it should always be his aim to reduce these approximations continually to closer limits. Certainly the approximations obtained by the methods of work which we have considered are extremely rough; to use them at all requires the admission of an extremely wide "limit of tolerance." For any finer work a much narrower limit is required. It is not at all surprising that as long as we are content with such broad approximations as alone can be obtained by the present methods the workers in the fields that require a higher degree of exactness, where the continual refining and scrutinizing of methods is insistently demanded, will look askance at our labors.

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[3] McAtee, W. L., *Entomological News,* XXXI, 62, (1920).

Assuming then that in any group the segregation of species has been carried as far as possible by the conventional means which we have discussed, what more can be done?

An excellent discussion of this question is to be found in the volume by Hall and Clements, *The Phylogenetic Method in Taxonomy,* to which we have already referred. Remembering again that although these authors are discussing the problems of botany their conclusions are equally applicable to any field with such modifications as the nature of the material requires, we may apply their consideration of the question to our own field.

It is the contention of these authors—as it is the contention of the present writer—that no final and scientifically valid conclusions as to the nature of species can be arrived at on the basis of preserved specimens. They say:

> The conviction has repeatedly been expressed that taxonomy to be phylogenetic and permanent must be based upon the field as the primary seat of investigation .... The field here is conceived in no narrow sense, but it includes greenhouse, garden, field enclosure, and in fact all habitats, natural or artificial, in which the evolutionary behavior of living plants can be studied. The complete field method demands a knowledge of the variation of the species concerned throughout its climatic area and of its adaptation as it enters diverse habitats adjacent to the normal one. It further requires statistical studies of the species and its variads in the whole range of natural conditions, and experimental analysis in controlled natural habitats as well as in garden and greenhouse. .... As a consequence it is little adapted to the preliminary organization of the flora of new or little known countries, but it is indispensable in testing and refining such results to the point where they accurately represent the facts of evolution and relationship.[4]

Concerning the application of statistical methods to systematic work these authors in a contiguous passage have the following to say:

> The statistical study of the degree of modification, as shown by the individuals of a species or a variad, yields results of the most fundamental importance, an importance surpassed only by those obtained from experiment. The first of these is a far more exact expression of the stage of evolution attained collectively than the most accurate judgment is capable of, together with a clear indication of the relation to adjacent units as revealed by the bases of the curve. Of equal importance is the evidence furnished by the modes as to the direction of the evolutionary movement. .... However, the chief value at present lies in the application to the study of variation as it occurs in nature, owing to the enormous mass of material demanding analysis. In this it is of the greatest value in determining whether a segregate represents an actual fact of evolution and in confirming its suggested relationship to the species as well as the nearest

[4] Hall and Clements, *op. cit.,* p. 19.

variads. While the statistical method applies only to characters that can be measured or counted, or to sequences, these are found to include practically all characters of importance in species and variads . . . . As statistical methods come more into use in evolutionary taxonomy, the results will be regularly expressed in biometrical form, but at present tabulations and simple graphs furnish all the facts required.

As an example of the most refined and elaborate application of such methods to what is in all essentials merely a systematic problem we may refer to the papers of Karl Pearson, dealing with biometrical studies of the bones of the human skeleton. In the course of this work he compares by means of such methods the bones of man and other primates, arriving as a result at far-reaching conclusions as to the relationships of the various species. This work is regarded by physical anthropologists as a most notable contribution to their science.

It is beyond the province of this volume to present any discussion of the general field of biometry as applied to systematic work. To do so would require a knowledge of the entire subject that would permit the selection of such methods as are directly applicable to our problems. Any such undertaking is a task for some one who is trained in both fields and who, consequently, can properly appreciate what is necessary in each. This the author makes no pretense of being able to do. It requires, however, no special training in the methods of the statistician to see that those methods can be of aid to us.

The application of experiment, on the other hand, to our problems is comparatively simple, even if time consuming. Concerning this Hall and Clements remark:

The rôle of experiment in evolutionary taxonomy is even more fundamental than that of statistics, though in the beginning it rather supplements the latter, owing to the time involved in complete experiments. Its unique importance lies partly in the control possible through the entire process, but it arises chiefly through the fact that it alone yields conclusive evidence as to descent. . . . . It must be the final arbiter in all questions of descent. . . . . The primary methods in the field are transplanting, planting, seeding, and modification of the habitat . . . . The first great objective of the experimental method is to determine the relationship by descent of the species and variads already in existence. In doing this it necessarily deals with the production of new variads under known conditions, and this leads to the study of the whole question of the causes and methods by which new forms arise. The latter opens up the fundamental problems of the origin and transmission of new characters, the solution of which is possible only through the widest range of experiments under measured conditions. In close relation to the origin of forms stands the experimental study of the criteria employed to distinguish variads, species, and genera. This is of particular value to existing taxonomy, and hence is one of the first as well as

one of the easiest points of attack. In this the correlation that exists between organs and parts offers one of the most promising of objectives and one that may serve to explain structures whose origin is now entirely obscure. . . . . While any of these problems may serve as the entry to experimental evolution, they are all so interrelated that a comprehensive attack upon them alone promises to yield adequate results in a reasonable time.[5]

The possibility of the application of such methods to systematic entomology is clear. Let us consider merely one simple case in which it might yield most interesting results.

The scale insect, *Lecanium corni* (Bouché), occurs in California upon a wide range of hosts.

That is to say, there occur in California upon a wide range of hosts scale insects of the genus *Lecanium* which can not satisfactorily be separated by the conventional methods from *Lecanium corni.* In certain respects, however, they differ quite markedly upon different hosts; for example, specimens taken from some hosts are very small, shiny, and light-colored, while those taken from other hosts are larger, darker colored, and covered more or less with powdery secretion. Structurally all these forms appear to be the same.

The question then forces itself upon the student of the Coccidae, what is the actual relationship of these forms? Are we really dealing with several things which are separate entities—species—or are these various forms merely variants of a single species, differing in appearance because of differences in environment? Within certain limits it is possible to see a direct influence of the host upon the insects for individuals that occur upon broad, flat surfaces will have a different shape from those forced to develop upon a rough or narrowly cylindrical surface. But whether the influence of host extends beyond such possibilities remains to be determined. Carefully controlled transfers from one host to another and the careful observation of the results would in all probability throw such light upon the question as to permit its satisfactory solution. If it appear, as a result of such transfers, that there is an actual modification of form, and perhaps also of physiological activity expressing itself in differences of color or amount of dermal secretions, we may regard the forms definitely as a single species. On the other hand, if the habit differences appear to be constant we may be led to seek further for small but significant structural differences which will permit us to separate the various forms as species, or we might utilize average differences and place them as races.

As a matter of fact such work is badly needed in entomology, for

[5] Hall and Clements, *op. cit.*, p. 20.

a priori conclusions as to the effect of differences in host have influenced the judgment of systematists in certain groups to no small degree. How ill-founded such judgments have frequently been may be judged from the fact that in some cases differences in host alone have been utilized as ground for separating species, while upon other occasions they have been used as a reason for uniting them, any differences in the insects being ascribed to difference in host!

Such work needs to be very carefully done and the results correlated with the facts to be obtained from other sources, notably from the external structure of the insects; otherwise some most peculiar results may be obtained, for there are many curious and at present inexplicable circumstances connected with the subject. For example, there is the fact that what is almost certainly the same species will in some localities attack a certain host while in another region it will refuse to feed upon such a host.

An example of the application of such experimental methods to a disputed point may be found in Nuttall's studies on the lice of man. He has shown very clearly[6] that the two supposed species, the head louse and the body louse, which occur upon man are simply forms or, at the very most, races, of a single species, it being possible to produce one form from the other. He has shown also by such methods the absurdity of systematic work which recognizes not only these two forms as distinct species but goes so far as to name a subspecies of each from each of several different races of man.

There are to be found in our literature numerous references to the question of the existence of "physiological species," that is, of forms which can not be separated on any structural basis but which behave in one way or another as distinct species. In the consideration of such cases the question of structural differences should first of all be carefully examined, for there exists always the possibility that something still remains to be discovered. But if such forms are found actually to exist—and it really seems that they do—there is a most important field for the investigation of their differences by means of physiological studies.

It can readily be seen that such methods might throw much light upon forms which are distinguished merely by food preferences, or by differences in rate of development or in the number of generations per year and the like.

It is also conceivable that chemical studies may at times prove useful. For example, a chemical examination of the composition of the various

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[6] Nuttall, G. H. F., "The Biology of Pediculus humanus. Supplementary Notes," *Parasitology,* XI, 201–220, (1919).

kinds of secretions to be found upon the scale insects might throw light upon the relationships of certain forms.

There is a place even for histological studies in such work. In the scale insects it appears that the various types of dermal pores and ducts, from which the secretions that are such a conspicuous feature of the members of this group arise, are of the highest systematic importance. But those pores and ducts can not be properly understood and classified without a knowledge of the histological structure of the glands associated with them. In such a case the histological investigation of these structures is just as much a part of the work of the systematist as is the examination of the insects by any other means.

Unquestionably genetical studies will in some cases aid in understanding the relationships of forms which would otherwise remain obscure. The ramifications of such studies into the work of the systematist are extensive and we shall refer to them again later.

All of these things then may be utilized by the systematist in arriving at conclusions as to the nature of species and are a part of his field to the extent that they do so. But even if he employ any or all of them as aids he still has before him the question of what a species really is.

In the first place, is a species an actual biological entity or is it merely a concept? The answer to this question probably depends much upon the interpretation of words. Hall and Clements in discussing this question say this:

> Apparently the general opinion is that the species is a concept, but this seems to deal with the term and its application rather than with the unit itself. Once granted that the term might have been applied to any other unit, or an altogether different term employed, it seems evident that the unit itself is as definite an entity as a plant community.[7]

The question may seem academic, and yet it is extremely practical. If it be considered that the species is simply a concept it may as well be admitted also that our conception is subject to change and represents nothing more than a conventional arrangement that will vary as our conventions vary. If for the moment restricted species happen to be the fashion, that is what a species will be. Or if for the moment the desire for a return to the broad Linnean concept happens to be predominant, we shall have species of that kind. At any rate we are not at all freed from the necessity of defining our concept. On the other hand, if the species is an actual biological entity there would seem to be some fixed basis to which it can be reduced.

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[7] Hall and Clements, *op. cit.*, p. 9.

Such fixed bases have, indeed, been suggested. From the point of view of the geneticist gametic purity is such a basis. But to the adoption of such a definition there is serious objection. These objections have been voiced by Hall and Clements, who say:

> It is a concept of use to geneticists alone, however, since gametic purity can only be determined by genetic analysis, and this is applicable so far to relatively few cases. Plants entirely identical in all external characters may exhibit a different gametic composition, as indicated by their behavior when hybridized, and would accordingly constitute different species. . . . . It is obvious that such a definition of the species is wholly impracticable, and that genetic analysis can be of value taxonomically only in studying differentiation within the species itself.[8]

Another fixed basis has been suggested by McLeod, from whose book, *The Quantitative Method in Biology,* we have already quoted. This writer takes the view that each animal and vegetable species differs strictly from all others by the chemical composition of its living substance. Such a definition is probably quite consistent with modern biology which thinks in terms of chemistry and physics, but, whatever theoretical value it may have, it is obviously utterly useless for practical purposes at the present time and probably for a long time to come.

Hall and Clements present the following statement of their views:

> The evolutionary view of the species is that it is a definite phylogenetic stock, sprung from and related to similar stocks, and itself undergoing modification into a number of variads. As they have recently come from the same stock, these variads are more nearly related to each other than they are to those of any other species, and they represent a definite phylogenetic unit, the species, at the same time that they mark its further differentiation.[9]

This is a broad view of the species, and it is for such a view, as opposed to close restriction, that these authors contend.

And so out of it all we are still left with no actual definition of a species. It remains still a matter of point of view and of opinion. There are those who have said that the question is not worth bothering about, for after all the species is a matter of definition. But this would not seem to clear the situation greatly, for it remains to be decided what that definition shall be.

But before attempting to present any definite expression of opinion it is necessary to consider certain other related questions.

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[8] Hall and Clements, *op. cit.,* p. 9.

[9] Hall and Clements, *op. cit.,* p. 11.

CHAPTER V

CATEGORIES LESS THAN THE SPECIES

Intimately bound up with the subject of the nature of species and the problem of their segregation is the matter of categories less than the species. In fact one can not be considered apart from the other, either biologically or nomenclatorially.

There are, as a matter of fact, two quite distinct problems which the systematist must consider at this point. The biological problem is that of recognizing these various forms and determining their genetic status, their actual relationships, one to another. This is the really significant and important problem. The other has to do merely with the giving of names to these categories and is not biological at all, except as the manner in which it is done may serve on the one hand to display the biological facts or on the other to conceal them.

It will be well to keep this distinction clearly in mind in the course of any discussion of the general problem and equally important to keep it in mind in the course of our actual systematic work.

If we accept the genetical view that a species is a gametically pure line we automatically eliminate much of the general problem, for there can hardly be any fractions of such a species. The only question would be that of what to do with gametically impure lines. But, as has been pointed out, this definition, in spite of its concreteness, is for any practical purposes unusable and this solution must, for the present at least, be dismissed from our consideration.

If we accept the point of view that a species is a phylogenetic stock with its variads, the variads in turn undergoing further differentiation, the problem of what to do with these secondary forms stands in need of solution equally with that of the species itself. To ignore these forms entirely, as some would have us do, is to conceal the very facts of nature which we need to reveal. To follow the advice of a certain biologist that we describe —and he unquestionably implied that we name—all the species that we can induce any reputable journal to print, is to go to the other extreme, for, as Hall and Clements remark, it is easy to discover and to describe these by the thousands. To strike a proper balance, to facilitate the understanding of such forms and not, on the one hand, to ignore them or, on the other, to make of them simply a chaotic and hopeless mass of meaningless detail, is the task of the systematist.

These minor forms are of many kinds. The simplest type is that which arises from the direct impress of the environment upon a single generation. Such is dwarfing, due to underfeeding or crowding, or differences in color

tone—melanism and xanthism—induced by extremes of temperature during development. No one has yet succeeded in causing such modifications to be transmitted from one generation to another and they have no genetic significance. Nevertheless, they may serve as traps for the systematist.

There are other types of normal variation which have no apparent cause. The extremes of such variation may differ so greatly that if taken by themselves they would appear to be different species. Such, for example, are the differences in segmentation of the antennae which are to be seen in scale insects of the genus *Pseudococcus* and related forms, which were long relied upon for the separation of species. It has been shown by one student that in some of the species of this group there was more variation in ten antennae representing a single species than had been recorded for all the species of mealy bugs. Such variation is everywhere present and may affect any part of the body. Whatever may be its origin and whatever of interest it may have as offering a possible foothold for the action of selective processes, it probably has no genetic significance. The study of such variation, however, should form an important part of the work of the systematist, for reasons which we have already pointed out.

There are those variations which are variously called "sports" or aberrations or mutations, that is, forms differing definitely by discontinuous stages from their parents. Such, to use an extreme case, would be a white crow, offspring of normal crows. These mutations are the material with which various geneticists have especially concerned themselves and from which, according to the views of the modern biological school, species probably arise. It is then evidently important to recognize such forms and to establish their genetic status.

There are also those forms which appear in di-morphic or poly-morphic species. Such are the winged and wingless generations of aphids and the short-winged and long-winged forms of various other Hemiptera. Doubtless such forms have at times been named as distinct species, and the possibility of their occurrence and the establishment of their actual relationships are important things to be considered in systematic work.

Such things as color forms are well known, both among the vertebrates and among the insects. Among the mammals there are for example the black and brown bears which may be offspring of a single parent, or the black and red phases of certain squirrels. McAtee has discussed the occurrence of these forms among the insects.

Color forms that occur more or less throughout the range of a species are frequent among insects, apparently more so than in any other group of animals. In mammals melanistic forms occur, in birds these dark forms also are found, as well as in certain cases reddish and grayish forms, but the practice has been

not to recognize these in nomenclature. In the case of color forms, however, it seems necessary to go a step farther in entomology than has been done in the taxonomy of the warm-blooded animals.

A very cogent reason for believing certain types of color varieties of insects to be distinct in character from color phases in vertebrates and therefore requiring different taxonomic treatment, is the fact that the insect body is composed of highly distinct (and, so far as normally colored surface is concerned, of entirely separated) parts which may vary independently in color, and to which certain colors may be sharply restricted, conditions vastly different from what may be observed in connection with the continuous body covering of most vertebrates. Two individuals of the eastern ruffed grouse, for example, may agree in practically every detail of color pattern, yet one be distinctly gray, the other decidedly rufous in the general tone of its plumage. This type of color variation is represented among insects by the red and yellow color phases of various species of *Erythroneura* (Jassoidea; Eupterygidae). Nevertheless these very insects exhibit another type of color variation in which certain markings may occur on the pronotum, scutellum, or other division of the body, in a certain series of specimens of both sexes, and be entirely lacking in another series, all of which, however, belong to what is considered a single species.

It being true that very distinct color forms in a structurally homogeneous complex, the species, are not unusual in insects, sorting and naming them would seem to be demanded by the practical considerations of making a collection appear to have been really classified, and to have names by which the forms can be referred to in exchanging and recording notes upon them. If they are left unnamed no phase of studying them will advance so rapidly, nor will final understanding of them be reached so soon, as will be the case if they are described and named.[1]

With certain of these arguments the present writer cannot agree, these having to do with the matter of naming such forms; but to this we shall revert later.

There is another group within the species which is of very great biological significance, and which is known to the mammalogist and the ornithologist as the subspecies. This is "a geographic race, a part of a species marked by average differences in characters which intergrade with those of subspecies occupying different, though usually adjacent parts of the general range of the species, along the common boundary of which intergradation is complete."[2] The term has been employed by entomologists but not usually in such a precise sense. Among the mammalogists and ornithologists it is very widely used.

The essence of the subspecies is inter-gradation, assumed or actual.

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[1] McAtee, W. L., *Entomological News*, XXXI, 51, (1920).

[2] McAtee, *op. cit.*, p. 47.

We may quote further from the remarks of McAtee, who is both an ornithologist and an entomologist:

Carried out logically, recognition in taxonomy of the principle of intergradation decides another much debated point—the status of island races, or even of those inhabiting separate continents. If individual variations are such that overlapping of all the differential characters occurs, the forms should be regarded as subspecies; if not they are properly considered as species. These conclusions, also, are not universally accepted. To some naturalists, obvious isolation as of island forms, and real or assumed isolation of continental races, apparently is taken as proof of specific distinctness. For the purpose of throwing light on this view we may consider the case of certain birds of the perennially interesting Galapagos Islands. Within the limits of a single and evidently very plastic genus—*Geospiza*—there are species confined to a single island, or to two islands, and so on, up to eleven islands in the highest number recorded by Snodgrass and Heller. What is a more cogent argument in the case, there are also *subspecies* that occur in just the same way, some of them upon only a single island, others upon two or more up to a maximum of eleven. Here is definite proof drawn from forms wholly of one plastic genus that a single subspecies actually ranges throughout a considerable number of well-separated islands, to certain of which at the same time, other subspecies and even species, among its congeners, are entirely restricted. In other words, the case proves, that isolation in itself is not admissible as a decisive factor in giving specific rank to organisms.

Subspecies [he continues] have not been very extensively used in entomology, for the sufficient reason that existing collections, in most cases, do not furnish enough material to enable the student to decide whether intergradation in characters of recognizable forms does occur, or if it does whether the forms have more or less separable ranges. However, work should be directed toward the recognition of subspecies, for the subspecific system of nomenclature is of too great value in expressing the facts of relationship and geographic distribution to be ignored.[3]

With this statement the writer of this volume is entirely in accord. It would profit the student of systematic entomology to study the work of ornithologists and mammalogists in order to gain an appreciation of the possibilities of this system. It may be added that such study might further be profitable by reason of the fact that undoubtedly some of the best systematic work which has been done is to be found in the literature of mammalogy.

Another term, "variety," has been quite extensively used in entomology. Unfortunately it has been employed very loosely and there is no general agreement as to its significance. It has, says McAtee, "been used to desig-

[3] McAtee, *op. cit.*, p. 50.

nate variants in size, structure, and color and varying ranks of all these." In addition it has frankly been employed by at least one author, the describer of great numbers of species, "to designate a form the status of which is uncertain." The term has been so badly abused that it were well to consider carefully whether it is not best to refrain from using it.

McAtee has stated the case for the variety at some length.

> Summing up a variety in entomology, actually of less than specific rank, may be one of three things: (1) It may be a true subspecies or geographic race, present material being insufficient to decide the point; (2) it may be a color phase, that is albinistic, melanistic, erythroic, or the like, an appearance it may assume anywhere in the range of the species that may affect all subspecies alike (proof of its nature) but which usually is recognizable as a phase of a simple color gradation, often as the alternative of two color states as albinism and melanism, and it should not have a name that will have to be reckoned with in scientific nomenclature; and (3) it may be a variety such as is known in many insects that cannot be subspecific in its nature, because unrelated to distribution, that does not answer to the definition of color phase, here given, but the real nature of which is not understood. It seems to the writer that these varieties have the importance, and in a way the attributes of subspecies except correlation with geographical distribution, and that they should be named. From a purely nomenclatorial point of view the fact that we do not know what color varieties really are is unimportant, and the writer's contention is, that we shall be much longer learning what they are, unnamed, than if named.[4]

He says further:

> Some insect varieties may have a genetic significance which when understood will indicate the proper taxonomic treatment. It is possible, even probable, that some varieties may be found, also, to be correlated with definite food plants or peculiar local habitats. Proof of such a relation would call for recognition of a new type of subspecies in which the geographic element of subspecies, as at present recognized, would be replaced by an ecologic factor—in other words it may become necessary to recognize two sorts of subspecies—namely geographic and ecologic.

We have then all these various forms, some of them certainly less than species, by any definition of the word that may reasonably be accepted; some of them possibly to be regarded as species upon the basis of a restricted definition of the word; some of them possibly representing what have been called "incipient species." And all of them are a source of doubt to the systematist. What are we to do with them?

One thing seems certain. We may not neglect them. If among these things there are to be found the materials from which species arise—

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[4] McAtee, *op. cit.*, p. 53.

whether that be by mutations or by some other process—to ignore them is to ignore some of the very things that give to the work of the systematist any philosophical value. If the species of the systematist are really to mean anything it can only be by the examination of such forms that any meaning can be given.

Out of it all there is the possibility of formulating certain principles that appear to be of general value. There follows an attempt to express these in categorical form.

1.

The recognition of and determination of the genetic status of the various forms associated with species and possibly minor to them is a basic part of the work of the systematist.

2.

This part of his work should not be confused with the problem of the giving of formal scientific names. The two are distinct things.

3.

The giving of formal scientific names, and the fixing of formal categories should be controlled by the aim of revealing broad facts. Those facts may be concealed either by including them within too large a mass or by isolating them from their proper connections.

4.

Certain practical considerations must be admitted in the giving of names, these having to do with the fact that our system of nomenclature must be kept usable.

5.

On these bases forms should not be named if the characters upon which they rest are not perpetuated from generation to generation.

6.

Forms based upon characters which may be perpetuated from generation to generation in accordance with the established laws of heredity, that is, characters which are mere dominants and recessives that have no opportunity to segregate into distinct groups or individuals, should not be named. Of such a nature are sports, or aberrations, or mutations that appear merely here and there in isolated individuals.

7.

Dimorphic or polymorphic forms, of whatever nature, derived from a common parentage should not be given formal scientific names. Of such a character are the various forms of aphids, such things as short-winged and long-winged generations, or color phases.

8.

In general, until the actual genetic status of doubtful species has been established or is definitely indicated, it were better to consider recognizable forms as species. The term "variety" because of the abuse to which it has been subjected and its almost complete lack of real meaning should be avoided.

We may accept then as our working definitions the following, considering them always in the light of the principles which have previously been enunciated:

If a group of closely similar individuals, or an individual which may represent such a group, is distinguished from another by one or more non-intergrading structural characters, it may be regarded as a species, unless and until it can be shown not to merit that distinction.

If such a group of individuals intergrades with another group from which it has a more or less distinct range this usually being interpreted as implying geographical separation but possibly to be extended to include physiological separation as well—it is to be considered a subspecies.

In general a clearly recognizable form is better to be called a species and so regarded until its actual status has been established by more refined methods.

CHAPTER VI

THE MORPHOLOGICAL BASIS OF SYSTEMATIC ENTOMOLOGY

Owing to practical considerations, as well as to the facts of biology, the greater part of our work in systematic entomology must be based upon the study of external structures, that is, upon morphology. It follows that morphology and systematics are essentially synonymous and inseparable. A genuinely scientific system demands that every structure which is to be found upon the body of the insect be taken into consideration and until that has been done our work is to be regarded as incomplete.

We have already pointed out that, however desirable and necessary it may be to take into consideration the evidence from every available source, the fact remains that most of our systematic work must continue to be based upon the evidence which may be derived from the specimens themselves. There are two simple reasons for this. One, already emphasized, is that such evidence is usually all that we can get. The other is that, however important the evidence obtainable from other sources may be in aiding us to determine what are and what are not species, it is usually of but little use in enabling us to *identify* them.

There is a third reason which is somewhat more complex. This is that the evidence to be derived from other than morphological sources has usually but little bearing upon the problems of classification of groups above the species. Useful as such evidence may be in aiding us to decide upon the genetic status of any particular form it will usually be of little aid in placing that species in a higher group. It is true, however, that sometimes such evidence may be of assistance.

There is, for example, a certain group of scale insects constituting the rather large genus *Kermes*. The members of this genus, with a single reputed exception, occur upon oaks. A single species, ascribed to this genus, has been recorded from *Acacia* in Australia. Here the biological evidence alone is sufficient to indicate, what the morphological evidence proves beyond a doubt, that the Australian species has nothing at all to do with the genus *Kermes*.

But let us look at the other side. There is another genus of scale insects, *Antonina*, every described species of which occurs upon grasses. There is another genus, *Sphaerococcus*, the type of which occurs on trees of the genus *Casuarina* in Australia. On the basis of the biological information there would be no reason to associate this *Sphaerococcus casuarinae*

with *Antonina*. Yet the fact is that this species, when examined morphologically, proves to be either a species of *Antonina* or at the most doubtfully to be excluded from that genus.

These are simply two examples. Almost every systematist, probably, will agree that evidence of this sort is at times extremely useful as an aid in the formation of conclusions as to relationships, but that taken by itself it is entirely too erratic and too dangerous to be relied upon. The same is true of other sources of information as well.

There is then the greater necessity that the systematist shall utilize to its fullest possibilities such evidence as can be obtained from the organisms themselves. In achieving an understanding of the larger groups undoubtedly the internal anatomy must be considered equally with external structure. Also in many cases no correct understanding of the external structures can be gained without consideration of the anatomy that underlies them. However, there is the practical difficulty that the internal structure can probably be studied thoroughly only in a relatively small number of cases and also it is often too constant and stable to aid greatly in the classification of closely related forms.

It is generally accepted by systematists that the external structure of insects is a trustworthy guide to the degree of their relationship and our classification is built up on that basis. To what extent such a system is actually valid may be debatable. To be valid at all it must rest upon the assumption that species have evolved into other species and groups into other groups by an orderly and regular process, structures being molded into different forms in such a manner that there is an uninterrupted continuity throughout the entire series. It is only on this basis that we may assume that extent of differences is actually a measure of relationship.

If, on the other hand, evolutionary processes occur through a series of quite irregular and discontinuous stages, a series of mutations let us say, our assumption that degree of difference is a measure of degree of relationship may be valid only in a broad way or even not at all. Let us consider an entirely possible example. Let us say that we have a series of flies, one with long wings, others with wings of various lengths and finally one that is apterous, these flies being closely related on the basis of other characters. Ordinarily a systematist would assume that the apterous fly was derived from the long-winged fly through the series of progressively shorter-winged forms.

Yet such may not be the case. It may be that the apterous form originated in a single mutation from the long-winged form and that the other apparently intermediate stages are not at all a part of a genetic series. Each of them may be produced independently as the result of

the action of a different factor. *In such a case degree of morphological difference would be not at all an indication of degree of relationship.* Exactly this sort of thing would seem to be indicated by the studies upon *Drosophila.*

We are confronted, consequently, by a genuine and serious difficulty. If a classification based upon morphological evidence may not be relied upon to express degrees of relationship we are back again in the pre-evolutionary situation where classification could hope to do nothing more than express degrees of apparent resemblance. All of our talk about relationship becomes futile. And yet before we hasten to discard our belief in the essential validity of our attempts thus to form a biologically truthful classification let us speculate a bit further.

Assuming the validity of the general theory of evolution, it will probably be admitted by every one that flies are more closely related to each other than to beetles. It will be admitted that probably Culicid flies are more closely related to each other than to other flies. It will be admitted that probably the species commonly referred to the genus *Culex* are more closely related to each other than to the species of *Anopheles.* Then if we may trust the morphological evidence up to this point, at what point does it become untrustworthy?

But, on the other hand, if we may not trust the morphological evidence in any particular case as far as it refers to the degree of relationship of individual species, but we may trust it as far as it refers to the larger groups of species, at what point does it become trustworthy?

One way out of the difficulty is to adopt the view that after all species do not normally arise by entirely disconnected mutations, in spite of the proven fact that such mutations may at times occur. However much the experimental worker may hold to a belief in mutations as the prime factor in species modification, the fact remains that the systematist may be equally impressed by the continuity of the thread of relationship. On the whole, there is too much consistency in the apparent connections between the various kinds of organisms to admit of a belief that development has been thoroughly erratic, even admitting that inconsistencies undoubtedly occur.

Another way is to assume that the two apparently conflicting groups of evidence can be reconciled. It may be that discontinuous and disconnected changes in form occur, giving rise to new species, but perhaps those changes are so limited in degree and in number that they will not in general obscure the essential connections. Among a group of normally winged species we may find a species that is wingless and which may have arisen by mutation, but the total assemblage of structural characters still

indicates its relationships. Certainly there are some things that mutations may not do—at least as far as producing species that endure is concerned. Mutations have not given to any enduring species of insect six tarsal segments or four pairs of legs or two pairs of antennae or more than twelve abdominal segments or any of a great many other things that might be mentioned. Nor, in spite of the amazing plasticity of insect structure and the extraordinary modifications to which that structure has actually been subjected, have any insects been so modified by mutations or any other medium that their essentially insectan character can no longer be traced.

Whatever be the actual answer to the problem which seems to have been evoked by the work of the geneticists, the fact remains that we are compelled to assume that a morphological classification, arrived at with due regard to all scientific precautions and safeguards and with due recognition of the possible sources of error, is fundamentally valid. We are not, however, freed from the necessity of checking and testing the results of such a classification by any other means that may be available.

The study of the external structures of insects constitutes the study of morphology. Given its broadest interpretation such a study should involve not only an examination of the external structures of mature insects, but also an examination of the way in which those structures have come into being in the ontogeny, the individual development, of these mature forms. For any proper understanding of comparative morphology the study must be carried back through the course of this individual development far enough to permit a clear understanding of the facts. It thus encroaches upon embryology, at least upon the later stages of development in the embryo.

It is perfectly true that all systematic entomology is theoretically based upon these external structures and certainly all systematic entomology makes use of these structures to a greater or less extent. Yet it is a strange circumstance that entomological systematists are frequently disposed to consider that the study of morphology is a thing entirely apart from the study of systematics. It is a fair statement, with which at least those whose specialty is morphology will undoubtedly agree, that the average entomological systematist has but the most rudimentary knowledge of the comparative morphology of even the immediate group with which he may deal.

It is apparently the point of view involved in this which leads our institutions in which entomology is taught as a special subject to divide it into various sections with systematic work and morphology usually entirely separate. Thus it is possible in some of these institutions to

"major" in systematic entomology and "minor" in morphology, or vice versa. It is apparently this point of view which led to the remark, voiced to the writer by one of the older systematists concerning a certain piece of work, "But that is morphology, not systematics." It is this point of view, one may surmise, that led to such a statement as the following, which appears in a very important paper of quite recent date: "We believe it much better for taxonomic work to designate a given area by a given name and call it that regardless of its possible homologies or analogies. As a system of designating veins and cells for the morphologist the Comstock-Needham system possesses certain advantages, but as a terminology to be used in taxonomic work we do not believe it to be desirable."

Why this artificial division between systematics and morphology should have arisen is difficult to see. The writer is inclined to associate it with factors having to do with the preparation of material for study, which will be discussed in a later chapter. However that may be, the division clearly exists, as is unmistakably indicated by the statement just quoted. It exists to the great detriment of our systematic work.

Leaving aside for the moment any consideration of the scientific basis of classification, the artificial character of this division becomes immediately apparent when we begin to study the current descriptions of insect species. In these descriptions, of which one will be quoted in the following chapter dealing with the preparation of material, there will be found mention of such features of the morphology—number of antennal segments, characters of tarsal claws, wing venation, and the like—as are readily visible; but beyond this the describer ordinarily does not go.

It is entirely pertinent to ask why this should be so. What determines the line between systematics and morphology? The systematist uses certain of the morphological characters, but not all. Why does he stop at the particular point which he chooses as the limit of his field?

The only answer which the writer can find to these questions is this: It appears that in general systematics are considered to involve those things which can be readily seen and interpreted, while all those things which demand some special study and the development of special methods of work and the possible dissection of specimens, are assigned to the morphologist. Such a conclusion seems entirely justified by the examination of our current systematic work.

Yet such a division is inconsistent with sound scientific principles and as long as it is adhered to will effectually block the progress of systematic work.

We have called attention to the fact that our classification is fundamentally morphological and that it assumes that differences in structure

are a measure of relationship. It assumes also as a fundamental concept that the possession of structures which come from a common origin is an indication of relationship, and this concept of common genetic origin of structures is expressed in the term *homology*. So firmly is this conception of the importance of homologies imbedded into the structure of all biology that it may be considered as one of the basic dogmas.

As one biologist has remarked: "Homology is no accident. All animals now possessing homologous organs must have descended from a common ancestral form. The organs of the present-day descendants of that ancestor arise in the same way in the embryo, because in some fundamental respect the organs of the ancestor arose in that manner. If homology is not an inheritance from a common ancestor, it can hardly have any significance at all."

Homology means then community of origin. This community of origin can only be positively demonstrated by the study of embryology. However it is out of the question to study the embryology of more than a limited number of forms and some substitute must be found. Comparative morphology, in the insects, furnishes that substitute. The examination of a long series of forms, even if they be all adult, will in most cases furnish evidence as to how the differences among them have been brought about, as to the course which evolution has followed, and, consequently, reasonably clear indications as to homologies. Even if the actual embryonic forms are unavailable, it may be possible to examine the later developmental stages and in many groups these stages will furnish evidence to fill the gaps between the adults.

So, properly interpreted, the morphology of the adults alone will go far toward establishing the facts of homology with sufficient clearness to admit of a reasonably adequate system of classification. But without such studies of comparative morphology no system that is worthy of the name can be devised.

Properly interpreted, the morphology of insects involves the study of every structure that is of ectodermal origin. The fact that a structure is obscure, or that it is invaginated within the body, does not properly exclude it from examination.

Consider then the unreasonableness of any system which would eliminate the study of structures from systematic work. Consider the unscientific character of a system which would draw an artificial line between those structures which are readily visible and those which are not, assigning one class to one branch and the other class to another branch.

Consider for a moment the implications contained in the quotation which we have cited above concerning the utilization of the Comstock-

Needham system of wing-vein nomenclature. Undoubtedly the development of this system was one of the most thoroughly splendid pieces of genuinely scientific work that have been done in entomology. It established the homologies of the wing veins throughout almost all the insects, and on this basis it has made possible a far more sound appreciation of the relationships and the lines of descent of the winged forms. Without it the study of fossil forms would be almost meaningless. And yet we see here advanced a claim to the effect that it were better for "taxonomic work to designate a given area by a given name and call it that regardless of its possible homologies or analogies." In other words, it were better to abandon the very principle which has supposedly been the corner stone of systematic work since the general acceptance of the Theory of Evolution!

Systematic entomology must be, in the main, simply the study of comparative morphology, together with the formulation of conclusions as to relationships on the basis of this study. Morphology may possibly have some limited aims that are peculiarly its own, such, for example, as attempts to correlate structure and function; but beyond this it is nothing but the study of the basic data of systematics. And any systematic entomology which ignores this fact is thereby cutting itself off from the right to be regarded as in principle scientific.

It may be replied that all this is true as far as it applies to the larger groups, but that it has no bearing on the description of species. Species may still be described, as they frequently are, with no consideration at all of anything more than the easily observable facts of shape, color and general proportions of the visible parts. To this the writer would reply that such work is not worth doing. It is from the descriptions of species that we obtain the data for the grouping of those species into the larger categories. To consider that the description of species, simply and solely as species, can be separated from the description of them as material from which the larger categories are to be built up appears to the writer to be distressingly fallacious. To accept any such principle would be to divide our systematic work into two quite distinct fields, one consisting purely of the superficial description of species, the other endeavoring, independently, with the aid of comparative morphology to arrange those superficially described species into a classificatory system. The absurdity of such an arrangement is obvious. Nevertheless much of our current work only narrowly escapes from that very absurdity.

There is but one logical escape, however, from such an end. That is, to regard the description of species as the means of bringing to light the data by which species are to be grouped. On this basis the description

is purely an exposition of morphology plus such other information as may be available.

Comparative morphology must take into consideration every structure that is to be found upon the body of the organism. If it be accepted that comparative morphology and systematics are synonymous it must follow as a fundamental principle that the same procedure must follow in our systematics. To accept any standard short of this is to accept an illogical and untenable position and voluntarily to limit the possibilities of our work. It is to accept a procedure that does not conform to the demands of scientific method.

Thus to see and to record the complete data of comparative morphology is to establish a fixed datum plane. We have then a point of reference that is but little influenced by the varying factors of judgment, personal prejudice, and the like, the influence of which we have already pointed out. It can be affected only by the individual ability to see clearly and to record definitely what is seen.

CHAPTER VII

THE PREPARATION OF MATERIAL

If a scientific method demands that every structure be taken into consideration, it follows as a necessary corollary that the material with which we work must be of such a nature as to permit the realization of this ideal. The development of the necessary technique in the preparation of material is a part of the requirements of systematic work.

This is a conception which would appear to need little urging, yet it is one that has had no extensive vogue among entomologists. The work of the entomological systematist has always been complicated by the small size of the majority of the species with which he deals and by other factors connected with the nature of his material. It is in fact quite safe to say that only of recent years has any special attempt been made to attain to the standard here expressed. On the one hand, it is only of recent years that technical methods have been developed which would make its attainment possible even to those who were at all interested, and on the other hand the general acceptance of the existing standards has made the development of such methods appear unnecessary.

The usual method of preserving insect collections for study has been the time-honored one of impaling the individual insects upon pins. This method has passed through various stages in its development and has been subjected to various modifications and refinements. It may be that the pin is extremely minute or that the insect is glued to a paper point which is in turn fastened upon the pin. It may be that the insect is impaled upon the pin with no special care to arrange its appendages, or it may be that, as in the case of butterflies, great care is taken to spread the wings in the most favorable way. It may even be that special apparatus and special methods are devised, as in the case of the Lepidoptera. But through it all the general principle remains the same. The essential feature is that the insect is preserved as nearly as possible in its living condition and it is consequently studied only as a whole.

This is the method primarily of the amateur collector. Its dominating interest is in the insect as a cabinet specimen. It is likely to regard the insect rather as an object of interest or of beauty or of curiosity than as a subject for detailed scientific study. In many cases at least, if not usually, it would choose rather to have the specimen intact and to know little or nothing about it, rather than to have it altered in its appearance, dismembered perhaps, and its beauty and interest as a cabinet specimen lost but with the compensation of increased knowledge.

It is a method which undoubtedly has its legitimate uses not only in the collections of those who gather insects merely for the pleasure of possessing them but even in the scientific collection as well. Leaving aside the special field of the Lepidoptera where elaborate and ingenious mounts have been devised, the pinned specimen does indeed offer the most practical —although not necessarily the best—way of merely preserving the larger insects. It is simple and quick and it has the advantage that it is easily adapted to the cursory examination of the more easily observable features.

But, unfortunately, the method has been utilized for something more than its proper field. It is the writer's contention that for genuinely scientific purposes, where the desire is to gain the maximum of knowledge, the method is decidedly limited. Nevertheless, it has been made the mainstay of even our supposedly scientific study of insects, and the development of that study has all too generally been limited by the possibilities of this particular method. Even in the case of the smallest insects—forms such as the lice, the fleas, the aphids, the scale insects—it has at some time or other been utilized, and even today the specialist in these groups will from time to time receive for identification material prepared in this way.

The pernicious effect of this method of study upon the development of our systematic work arises from the fact that it has been responsible for the setting of low standards. It permits the use of nothing but the lower powers of the microscope, and even with the aid of these but a relatively small proportion of the structures of the body can be seen. Its insistence upon the insect as an object to be preserved intact has led to the voluntary acceptance of a standard which demands nothing more than a partial and limited knowledge of the object.

If this statement be doubted let us consider the facts in connection with the study of some of our insect groups.

The great order of the Coleoptera is one of the most favored by collectors. It contains an enormous number of large and strikingly colored forms and it is consequently an order that is especially suitable for cabinet display. This has led to the circumstance that, with the exception of the Lepidoptera, it is probably the best known of the insect orders. The literature is voluminous and there are many students of the group. Yet what are the actual conditions?

It is quite fair to say that although this order has been studied with what has passed as great intensity by a long series of students—some of whom were undoubtedly masters—at least one-third of the total number of structures to be found upon the bodies of the insects of this group are practically unknown to the students of the order. Those obvious structures which can be seen in the ordinary pinned specimen are well known, but

beyond these the curiosity of the majority of Coleopterists has rarely penetrated. If the validity of this statement be doubted, let us consider an actual example which will illustrate the point.

There follows the description of a new species of beetle, taken from one of our leading entomological journals of the date of June 1926:

Elongate oval, convex, surface moderately shining. Color above and below black, with more or less bluish to purplish luster. Appendages black with some luster. Head with vertex smooth, finely alutaceous, frontal carina distinct, subacute, tubercles flattened, subtriangular, moderately distinct. Eyes small, their width as viewed from the front a little less than one-third the interocular distance, usually partly withdrawn into the thorax. Antennae slender, about one-half the length of the body in the males, shorter in the females, joints 2–4 successively longer, joint 4 twice the length of 2. Thorax about one-half wider than long, sides margined, distinctly narrowed in front, subparallel on basal half, front angles thickened, disk faintly alutaceous, extremely finely and sparsely punctate, ante-basal groove narrowly, not deeply, but distinctly impressed, quite straight, not reaching sides. Elytra not much wider at base than thorax, distinctly elongate-oval in outline, tapering behind middle to apex, humeri broadly rounded, umbones not prominent, surface distinctly alutaceous, uneven, somewhat rugose, punctures widely separated, somewhat coarser at base than those of thorax, very fine toward apex. Abdomen beneath alutaceous, moderately pubescent.

Male—Last ventral with a median lobe and a broad, triangular impression. First anterior tarsal joint enlarged.

This description is entirely typical of by far the greater part of the work on the Coleoptera and was selected entirely at random.

It is perfectly obvious that this description considers only characters which could be seen with a low power of the microscope. Neither does the description include any structure for the study of which it would be necessary to dissect or in any other way disturb the specimen. *It omits all reference to the posterior wings; to the characters of the dorsum of the abdomen, which is concealed beneath the elytra; to the genitalia of the male; to the genitalia of the female; to the details of the mouthparts; to any details of chaetotaxy; to the form of almost all the morphological elements of the body.*

And yet this description applies to a species belonging to a genus of which an authority on the Coleoptera, in a standard book of reference, says, "The genus is a difficult one, and the species can never be determined with any degree of certainty without a fair number of specimens, some of which must be males." Surely, it would seem that it is exactly in such a group that the application of the principles which we have here proposed is above all demanded.

It is, of course, true that these various structures which we have mentioned have at times been utilized by Coleopterists. There can be found descriptions in which some of these omitted features are discussed, but descriptions in which all of them are considered are extremely rare. Even in a group as well known as are the Cicindelidae the genitalia of male and female are omitted from descriptions, although the genitalia of the female, at least, offer excellent specific characters.

What is one to say of a system which contents itself with such an incomplete examination of its material? Can it be regarded as conforming to the requirements of scientific method?

The same question may be asked in regard to the work upon other groups as well. For example, in the Diptera the genitalia of the males—which are usually partially or entirely concealed—have been relatively little studied. In the Diptera Pupipara the conventional methods of study from pinned specimens have led to the result that the structures of one whole body region—the abdomen—have quite generally been practically ignored, for in pinned specimens this region becomes so shrunken that study from such specimens is impossible. One could continue through almost every group of the insects pointing out the results of such methods.

In those groups which are of such small size that study from pinned material is entirely impractical and which are unsuitable for cabinet display the development of other methods has literally been forced upon the systematist. In some of these groups a much closer approach to the ideals which we have expressed has at times been attained, although even here there remains much to be done. But it is from the students of such groups that improvement in the conventional methods has for the most part come.

When and by whom the utilization of the glass microscope slide and some sort of mounting medium was first attempted is not known to the author, but with its application to the smaller forms of insect life there was a distinct step forward in the study of these forms. By this means it became possible to employ the higher powers of the microscope and to observe structures that would otherwise have remained invisible. Even so it remained for long an inadequate method and the earlier observers were much handicapped by its deficiencies.

One of the oldest methods of such microscopic study has been that of mounting specimens in balsam, which is the resin of a coniferous tree. This is the most time-honored of all, for actually the beautifully preserved amber fossils are merely balsam mounts and the recent work of Crampton has shown what can be done with such material. But in slide preparations the method has decided disadvantages. For one thing, the balsam tends to turn yellow upon exposure to the air and in time the preparations become

so dark as to obscure delicate structures. For another thing, the refractive index of balsam is such that clear and transparent chitinous structures are rendered practically invisible.

So from time to time other media have been used. For example, glycerin jelly, which has the advantage that its index of refraction is such that even the most delicate structures can be seen, has been widely employed. This has the very serious disadvantage that such preparations can not be made permanent. Experiments have been made also with damar and euparal, but these substances have not come into common use.

At some time, someone discovered the fact that by treating the insects in a solution of caustic potash (KOH) it is possible to break down the internal soft tissues and thereby render the body clearer so that it can be studied much more easily by transmitted light. This process in its simplest form consists of merely soaking the specimens in such a solution. It was also found that by boiling the specimens for a very short time the same effect could be obtained. The next step was that of removing the disintegrated body contents, leaving nothing but the chitinous exo-skeleton. Thus freed entirely from the obscuring internal structures it was possible to see the various dermal structures even more clearly.

But still the method had its deficiencies. Many of the dermal structures are extremely difficult to see, especially, as has been pointed out, when mounted in balsam. The solution of this difficulty is that of staining the specimens with some substance which has an affinity for the substances with which chitin is impregnated. With the discovery of a suitable stain, that is of a stain which will select such structures of the body wall as setae and the like and areas of what is commonly called heavier chitinization, the ease and certainty of study has been greatly facilitated. In fact it appears that the method has at last been developed about as far as may be hoped for.

Through these various steps, together with the introduction of the binocular dissecting microscope and the development of a more refined and at the same time more practical technique of handling even the most minute specimens, the preparation of material in such groups as the scale insects, the aphids, the various ectoparasites of birds and mammals and certain other groups composed of small forms has at last attained to a stage where it may fairly be claimed that it is actually possible to see every structure that exists upon the body of the specimen. At least one cannot conceive of any mysterious way in which workers of the future will be able to see anything more. We are in certain of these groups in a fair way to the realization of at least a reasonable approximation to the ideals which we have set forth.

The general procedure employed in dealing with such insects as the Coccidae—the group in which this method has perhaps been most successfully employed—must of course be varied in its application to other groups, for in some respects the scale insects are very simple things to deal with. Basically the method remains the same, regardless of the group with which we are dealing, whether it be a minute first-stage scale insect or a beetle two inches long.

The insect, or the portion of an insect, that is to be prepared may be preserved dry or in some preserving fluid, or it may be living material. It is placed directly into a solution of caustic potash and water, an eight or ten per cent solution being the most satisfactory. The specimen is then boiled in this solution for a length of time that depends upon various factors, such as the manner of preservation, the size of the insect and the character of its integument. A minute dry specimen may possibly be sufficiently boiled in two minutes. Specimens preserved in a solution containing formalin seem to require a much longer time. The period is determinable only by experience. The specimens should be boiled only until the body contents are thoroughly liquefied. If the boiling is continued too long the integument itself is attacked and broken down and the specimen may be completely ruined.

The specimens should then be removed to a watch glass of water, under a binocular microscope, and a slit or opening made in the body. Through this opening the liquefied contents may gently be pressed out. If the boiling has been carried to the proper point this is a simple matter. The specimens are then transferred to 95% alcohol to wash out the caustic potash and water and then, if desired, are stained.

Gage has recommended the following formula as a satisfactory stain:

Säurefuchsin	0.5 gr.
Hydrochloric acid, 10%	25.0 cc.
Distilled water	300.0 cc.

The writer has employed magenta in the following formula:

Magenta crystals (basic fuchsin; rubin)	1 gr.
Alcohol, 95%	100.0 cc.

Actually there is probably little to choose between the two stains, although the writer has been inclined to regard the magenta as the more selective. The magenta is very sensitive to alkaline solutions and it appears that to insure permanency the final mounting medium should be acid.

Ordinarily about an hour in the stain is sufficient, although it may be desirable to leave them longer; and in the case of specimens stained in magenta they are removed to 95% alcohol and the excess stain washed out as quickly as possible. The specimens should not be soaked in the

alcohol for any length of time as the brightness of the stain in the tissues will be reduced.

The specimens are then passed through carbol-xylene—three parts xylene to one part carbolic acid crystals—and mounted in balsam. If the stain has been satisfactory the membranous areas will appear unaffected but setae and sclerotic areas will show as a clear red. The result is a most beautiful preparation in which the finest detail can clearly be seen.

This is the general method. It is obvious that in its employment many details must be observed which it were useless to discuss here. The method unquestionably has its defects. For one thing, very delicate membranous structures may be destroyed or modified by the boiling. Neither do such structures take the stain. On the other hand, heavily sclerotic structures may be so deeply stained or retain so much excess stain that they obscure those which are more delicate. In such cases it is necessary to vary the procedure.

In the case of heavily pigmented insects, such as fleas, it is evident that staining is not only unnecessary but disadvantageous. In other cases it may be desirable to dissect off various portions of the body and to treat them differently. This is always the case with wings, which will not bear boiling, for in the boiling the wing membranes separate and the whole structure becomes badly distorted. Wings, however, may be stained without previous boiling.

By suitable variations the method may be employed for entire insects of considerable size, such, for example, as the Diptera Pupipara. It is necessary to develop a certain amount of skill in handling the specimens in order not to mangle and distort them beyond all possibility of use. At the best some distortion is inevitable, but this can be greatly minimized by an intelligent study of each specific problem.

As an example of the refinement to which the method may be carried we may refer to the work of J. C. Chamberlin on the Pseudoscorpions. This author found that the method required much modification in order to be successful. He permits me to quote from an unpublished manuscript the following account of his procedure.

> The most serious drawbacks which this method [as previously described] possessed arose from the fact that boiling with caustic potash tended to destroy, or at least seriously distort, certain of the delicate lamellar structures of the chelicerae, and secondly it was impossible to stain the entire animal evenly, owing to the fact that the enormous pedipalps took up such a great quantity of stain that it was impracticable to wash out the surplus without entirely destaining abdominal and carapacal structures. Then too these organs [the palpi] stained so darkly that it was impossible to study the poison apparatus and tactile setae at all in many cases.

It was in the attempt to obviate these deficiencies that the following method was finally devised.

Legs I and IV, both chelicerae and both palpi, are first dissected from the body, which is then gently cooked the minimum possible amount in KOH, and afterwards stained in the manner previously described.

The previously removed appendages are next transferred from the 95% alcohol (in which the dissection was previously made) to carbol-xylene and allowed to clear for a period of time varying according to the density of the object to be cleared. In species having small chelicerae it is often advantageous to mount in balsam directly from the alcohol. The legs are then mounted with the chelicerae, providing there is not too great a disparity in their convexity. (If the latter is the case they should be mounted separately, as otherwise one or the other sets of organs will ultimately drift from under the coverglass.) In general the movable finger of one chelicera should be dissected off in order to make certain of the delicate structures of this organ visible. This facilitates examination of the flagellum in particular. Unless this dissection is successfully made it is very difficult adequately to study this systematically important structure. Possibly the best method for this purpose is not to disarticulate the movable finger but merely to split the palm into halves near the point of origin of the flagellum.

One palpus is mounted entire, preferably ventral side up. The other palpus should have the claw dissected off and the chela widely opened by means of a deftly inserted needle. These parts are then all mounted with the appendageless, stained body. In this latter mount (owing to the convexity of the palpi and body) the cover glass must be mechanically supported. This is accomplished by means of a washer-like ring, which serves to keep the enclosed objects from drifting from under the coverglass. It likewise serves to prevent the ingress of air bubbles.

Thus any one specimen consists of two to possibly four separate mounts. I usually mount these on one or two slides, two mounts to a slide. These preparations are as permanent as possible, there is no danger of insect pests, drying out, or of damage through breakage from ordinary handling. Above all, they permit adequate study.

It is of course desirable to supplement such preparations by the study of alcoholic material. Entire, uncooked, carbol-xylene cleared and balsam mounted specimens are also useful. Unfortunately this technique is itself not perfect and certain disadvantages early become apparent. For example, the genitalic structures are often displaced, and delicate portions are distorted and sometimes partially destroyed. The balsam clears the unstained and untreated material, particularly the delicate structures of the chelicerae. This latter difficulty occurs to such an extent that in many cases the lamellar structures become practically invisible after a time. Possibly they should be stained in addition, although I have not tried this.

The best—but often systematically impossible—way to study the male genitalia, is to dissect them carefully from an alcoholic specimen. In this case the abdomen is first dissected from the cephalothorax, an incision is then made the

length of the pleural abdominal membrane and transversely between the fourth and fifth or fifth and sixth ventrites, thus removing the entire abdominal contents. The viscera and attached ventrites should then be mounted, ventral side up, in a ring mount.

This will indicate something of how the method may be adapted to a special group. It is evident that the development of the process which is best fitted to any particular type requires a considerable amount of thought and experiment and demands a rather high degree of manual skill. Heinrich has said, "No entomologist is worthy of the name who cannot make decent preparations—slide or otherwise—of the insects he studies. An untrained schoolgirl can do it with a week's practice."[1] With the first part of this statement the writer will agree, but not with the second. In the case of forms where the procedure is of the simplest type and no especially elaborate technique is necessary it is possible to acquire the rudiments in a short time. But to produce satisfactory preparations of forms where any extensive modification of the basic procedure is necessary is another matter. This is a task for one who knows what the preparations are for. Without such knowledge the preparator is working in the dark.

The mounting of the specimens upon the slide will be regulated entirely by the necessities of the individual case. A flea is simple—it can be mounted in but one way. One of the parasitic Hymenoptera is difficult, for there are many parts which must be dissected off and arranged in the most suitable fashion to facilitate study. In mounting the specimens upon the slide it is frequently desirable to have at hand paper rings of various thicknesses which can be used to support the cover glass and prevent crushing, the ring sustaining the weight. In the absence of such rings the cover glass may be supported by bits of broken coverglass or thin glass slides. Owing to certain characteristics that balsam displays upon drying this method of mounting by bits of glass has decided defects.

The methods as thus applied are primarily for insects of quite small size. Their application to such forms has been tested sufficiently to admit of no argument as to their value. To apply them to the larger species is perhaps a different thing. Nor would the author maintain that a Hercules beetle should be treated in exactly the same way as a minute Coccid.

With these larger forms it may be desirable to relax the specimens and then to dissect off those portions which cannot adequately be studied in a pin mount. The mouthparts of almost all insects should be so treated. The same is true with the genitalia, both of males and of females, the minute details of which cannot be determined satisfactorily even in specimens

[1] Heinrich, Carl, *Proceedings, Entomological Society of Washington,* XXVI, No. 1, editorial page, unnumbered, 1924.

preserved in fluid and thus free from shrinkage. The essential feature is that such structures almost invariably demand to be studied by transmitted, rather than by reflected, light. There are numerous types of dermal structures which can be understood only by such treatment. These portions may be treated separately.

In the case of many, if not most, of these larger forms it will not be necessary or even desirable to stain the specimens. It may not be desirable even to mount the specimens on a slide for in the case of complicated structures, such as genitalia, it may be necessary to retain the power of manipulating the specimen. Consequently such specimens may be preserved in glass tubes. In the case of valuable material, such as types, the hermetically sealed glass tube could be utilized.

The point of it all is, however, that whatever method of *preserving* the specimens be adopted, *such cleared and dissected portions should be the basis of study*. The pinned specimen may remain merely as a convenient way of preserving large quantities of material in a form for cursory examination and to show such characters as color and habit. The actual scientific collection will be that which will permit genuinely scientific study. The practical difficulties of keeping the two types of collections correlated are of course to be recognized but are by no means serious. Even if they were so they should not be regarded as a deciding factor.

It will be noted that the employment of these methods demands a radical departure from the ideals which have ordinarily governed the administration of the collections of the larger insects. It necessitates a procedure which would ordinarily be regarded as "destroying the specimen." At least the specimen as an object of beauty has ceased to exist. Whatever color it may have had is gone. Its shape is probably distorted. It is perhaps completely dismembered. It has become a mere specter, floating in a bottle or encased in balsam.

But from the point of view of scientific study all these things are immaterial. A record of color can be made before the specimen is dealt with. Distortion can be allowed for. Dismembered parts can still be associated with each other. The important and essential thing is that the object is in a condition that will admit of complete study; a study which will include everything that is to be seen upon the specimen. Anything short of this, any voluntary limiting of the objective is in some measure failure. It may perhaps chance that after all this labor nothing of importance is revealed, but that fact cannot be determined on *a priori* grounds. Our curiosity should not terminate until every avenue of approach has been explored, even if the exploration prove fruitless. There will be but few cases where we shall be so poorly rewarded.

CHAPTER VIII

ENTOMOLOGICAL DRAFTING

The second step in systematic work is the recording of the data obtained in the course of the first in order to make possible its future use for the purposes of identification. In order to obtain a fixed basis, a fixed datum plane, it is necessary that all the data be recorded and that a technique be developed which will make this possible.

It has been customary, as we have pointed out, to describe species in words with a degree of detail that varies tremendously. The difficulties confronting the describer are many and various. As has been said by Harrison:

> The systematic worker is at present faced with two alternatives when he comes to write a description of a new species. Either he must write a very long and detailed description, giving equal value to an infinite number of characters, some of which he may find later to be very variable; or he may try, in a brief account, to found his species upon a few characters of absolute diagnostic value. In the former case the work is laborious, which would not be of any consequence did the result justify it; but the longest description very frequently fails to characterize one of two closely related species if it has been discussed without reference to the other. In the latter case there is at present no general agreement among workers and the opinions of one man as to what is of absolute diagnostic value will differ widely from those of another.[1]

Furthermore, if an author attempts to solve the problem by the giving of long descriptions he may succeed merely in obscuring the essential data under a cloud of words. So it happens that authors have been criticized both for giving descriptions that are too long and for giving descriptions that are too short.

Let us consider what it is that we are really trying to do when we describe a species. What is the real function of the description? We have dismissed the idea that it is merely to comply with the technical requirements of "establishing a name." What remains for it to do?

There are really two functions involved in the specific description. Of these the simplest and easiest to fulfill is that of permitting some sort of identification of specimens in hand. For this purpose a few well-chosen words may frequently suffice, especially when taken in conjunction with

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[1] Harrison, L. A., "The Genera and Species of Mallophaga," *Parasitology* XI, 7, (1916).

other circumstantial evidence. If, for example, a scale insect which is referred to a certain genus has been described from a certain host species in a certain locality and we obtain from that host and locality a species which is referable to that genus we may arrive at a reasonably positive determination on the basis of a very scanty description. It may in fact be a description that actually tells us almost nothing about the insect.

It is because of descriptions of this sort that a very large proportion of the work merely of determining specimens in hand is based upon little more than a theory of probabilities. In the case above cited—which is typical of the conditions in group after group—such a theory of probabilities is directly applied. There is in many cases no actual proof that the identification is correct.

But even if we were content merely with the possibility of identifying specimens quite positively there still remains something that we may legitimately demand from the descriptions. That is that they shall permit us to form a clear conception of the species even in the entire absence of specimens for study. This very few descriptions do. With the majority of them one is practically helpless unless a considerable number of determined species are available to begin with. It is this which makes it so difficult for the student to work with an unfamiliar group. His first problem almost is that of obtaining enough determined species to give him a basis for work. All of this is quite unnecessary. If the principle which we have enunciated, that everything which is seen upon the organism should be recorded be accepted and applied, the difficulty would either not exist at all or would be reduced to such proportions as to be relatively insignificant.

The second function, then, of the description is to make possible the understanding of a species even in the absence of material for study.

Consequently it may be considered that the description should stand as a substitute for the object itself. Upon the degree to which it succeeds in doing so depends its success or failure. To accept any standard less than this is to return to the voluntarily accepted limitation of our aims, from which we are trying to escape.

The attainment of this ideal may be regarded as impossible on the basis of words alone. No purely verbal description, with the hampering limitations that are inevitably associated with it, can thus form a satisfactory substitute for the object. It is possible to describe a simple geometrical figure in terms of words, but as these figures become more complex the difficulty is enormously increased. How much more difficult will it be then to describe an insect with its complex spatial relations of parts, its structures of varying form, its variety of curves, its various

axes, its different planes, its wealth of detail! The task is one for which words are an unsuitable tool.

There remains but one other way by which this ideal may be approached, and that is by means of some sort of pictorial or graphic representation.

The architect, or the designer of any machine or piece of apparatus or any complicated structure, does not rely upon verbal descriptions as a means of conveying his conceptions to others. Even the simplest of such work demands that the ideas be expressed in some form more precise, more complete, less capable of distortion, and consequently more objective than words. A design may be accompanied by specifications, but the work of reconstructing the object depends upon the designer's blue prints, upon a graphic representation.

The function of the systematic description, which we have already stated, is essentially similar. It is—or it should be—the intention of the describer to enable the user to reconstruct the object described, to learn from the description all that the describer himself knows about the object. The entomological systematist will find the methods of the mechanical draftsman equally applicable to his own subject.

And just as the architect who cannot prepare the designs for his building or who does not possess a drafting department in his office is almost inconceivable, so—it is the writer's conviction—will the time come when the systematist who cannot prepare the illustrations, which perform the same function as do the architect's designs, will become an anachronism.

The aim of the draftsman's blue prints is to describe in the most precise, the most convenient, the most complete, and the most readily understandable way the data which it is desired to pass from one mind to another. This method is the one way of accomplishing such a purpose that is most nearly free from purely subjective distortions both on the part of the describer and the user of the description. It converts the process of conveying information to an objective basis. It makes possible the recording of the minutest detail, free from all those difficulties which are present in the verbal description. It makes possible the showing of every structure and relieves the describer from the necessity of selecting those things which he will describe and those which he will ignore. It permits the setting, if not the actual attainment, of a fixed standard.

Furthermore, it presents the nearest approach to a universal medium of expression that is known to man. The language of pictures is understandable by the intelligent individual, regardless of the tongue that he speaks.

If these ideas be admitted as sound, another step also may be admitted.

It may be surmised that the technical methods which the mechanical draftsman employs are at least in part applicable to our own work. This the writer contends to be the case. After all, what is an insect but a minute piece of machinery? Is the problem of describing a watch, let us say, in such a way that another who does not have a watch in his possession might construct one, essentially different from that of describing a beetle?

In this matter of scientific illustrations the writer believes that there is much misunderstanding. It may clarify the question a bit to point out that there are two general functions for such illustrations.

If we are dealing with a popular book intended for general readers the illustrations serve the entirely legitimate function of lightening up the monotonous succession of printed pages and of giving to the reader a general idea of the subject. But if we are preparing a scientific paper, the users of which will be concerned only with the acquiring of precise information, we are concerned with a quite different affair. Yet the two functions are very frequently confused.

It is only with illustrations of the second type that we are here concerned.

As the engineering draftsman has developed a technique which admirably meets his needs, and as his needs and our own are very similar we may very well adapt to our system such parts of his as we can use. There are certain modifications which are rendered necessary by differences in the nature of the material with which we deal, but aside from these the engineering draftsman's technique may be transferred bodily to our own uses. We do not need, however, to concern ourselves with the mathematical phases of his work.

The principles of the engineering draftsman are very simple.

To begin with, the draftsman is supposed to know how to handle his tools. There is no excuse for sloppiness. A very large portion of the current entomological illustrating does not attain to this standard.

The work of the draftsman "has one great purpose, and that is to be useful. To this end lines may be added or left out, shading may be used or notes may be put on. Almost anything which will make a drawing more readily intelligible is justified."

To secure the greatest intelligibility and the greatest ease of interpretation—of reading—it is necessary that simplicity shall always be sought for. To this end his drawings are divested of every extraneous factor. Nothing that is not essential to the clear expression of the idea which he desires to convey is added. If we apply this principle to many of our current entomological illustrations we may see how little it has been

considered. It is all too frequently subordinated to the desire to make a "picture" and all too often these labored strivings to be artistic end only in making the drawing neither artistic nor useful.

The draftsman seeks for accuracy, accuracy that is real and not merely factitious.

The draftsman's methods are a series of conventionalizations. Consequently draftsmen seek to attain a common method of conventionalizing and a general intelligibility.

All these principles are easily understood and are applicable in their entirety to our needs.

Photographs.—The author considers that photographs as applied to the needs of the entomological systematist are generally failures. The task of deciphering the complicated structures, on various planes, of the insect body is frequently sufficiently difficult for the eye, and then to show these structures in the clearest fashion is equally so. To ask the camera to do this for us is to ask it to perform a task that is beyond its powers. Some photographs, such as the very beautiful ones used by Davis to illustrate his descriptions of cicadas, are of use as a means of illustrating general appearance and habit; but beyond this they usually cannot go. They cannot be utilized for the clear illustration of morphological details in any but a very few special cases.

Drawing.—In spite of the circumstance that a large part of the work in many laboratory classes in entomology involves the making of drawings, the fact remains that the average student when confronted with the necessity of producing a drawing that will conform with the principles here set forth and with the technical requirements of reproduction is practically untrained and meets a situation with which he cannot cope.

It is not the intention here to present an elaborate discussion which shall serve as a text on the subject of entomological drafting in all its ramifications. The writer wishes merely to discuss the general principles and the general features of such work with an aim to aiding the student in seeing clearly the requirements and the general methods of procedure. Let it be understood that the author is not discussing the matter of "Art" as applied to such work. It is his intention to discuss merely drafting. The distinction should be kept clearly in mind. Whatever Art may be—and that is a matter about which there long has been and doubtless long will be argument—it is not concerned primarily with the presentation of facts. It is with this very presentation of facts that the draftsman is alone concerned. It may sometimes happen that there is combined in one individual the unusual capacity to be both artistic and accurate. There is in fact one such illustrator working today in entomology who is both

an artist and a superlatively accurate draftsman, but he stands almost alone. For the average worker it will be enough to content himself with being a good draftsman. That is, he will merely record with severely simple means and with the utmost accuracy what he may see. Yet even so such work should be done as well as the method employed will admit; within the limitations of a plain line drawing, the drawing should be made as well as possible. There is no excuse for sloppiness in our work any more than in the engineer's.

The medium of expression.—Owing to a number of factors the most practical medium of expression for the entomological draftsman, as well as for the engineer, is pen-and-ink line work. On the one hand, it is one of the simplest media to master. On the other hand, it is one of the cheapest types of illustration to reproduce.

The cheapest method of reproducing any illustration is that of the zinc etching or line cut. Owing to the way in which these reproductions are made nothing but solid black will reproduce and consequently the delicate shades of expression possible with a pencil or wash drawing are ruled out or must be obtained by manipulation of the pen. This requirement, that everything which is to reproduce must be black, must never be lost sight of by the draftsman. It is for this reason that pencil drawings cannot be satisfactorily reproduced by this method, for an ordinary pencil line will disappear entirely.

If pencil or wash drawings are to be reproduced, other methods such as lithographing or the half tone must be employed. Their cost is much greater than that of the zinc etching, and except in the case of photographs there is nothing gained by their use. We shall not consider these here. Neither shall we consider color. These things are outside the general scope of the draftsman.

For the making of line drawings only a jet black drawing ink, such as Higgin's India ink, should be used and care should be taken to see that the lines are not grayed by the presence of grease on the paper.

The Equipment.—The matter of pens is one of individual taste. Any good steel pen will serve. Whether one uses several sizes or a single pen for all purposes is a matter of no importance. Frequently it will be desirable to grind the pen points down until they give the desired line or dot. Ball-pointed pens may be used for heavy lines.

A ruling pen, of the type used by draftsmen should be available, as should also a good ruler, soft erasers, and a scalpel or other sharp instrument with which ink lines may be removed. There should be a liberal supply of thin paper, such as typewriter copy paper, thin enough to use for making tracings.

The drawing paper may be any one of several kinds, ranging from the rather high priced Bristol board to the thin laboratory-notebook drawing paper. The only fixed requirement is that it should have a surface that will not pick up on the pen and that will take a smooth line. It should be borne in mind that there is always much waste in using such paper, and it is not necessary to use high-priced material, for there is no corresponding gain, at least in ordinary work. It is sometimes emphasized that paper should be pure white, but the writer has long employed a cream-colored paper, the drawings apparently reproducing quite satisfactorily.

It should always be remembered that the ideal is accuracy. Consequently every aid to accuracy that is available should be employed. For the making of drawings of microscopic objects the eye is never sufficiently accurate and the camera lucida or some form of projection apparatus should be employed. In the case of objects too large for the microscope there should be careful measurements, or the drawings may be made with the aid of apparatus designed for such objects. We are not at all concerned with any of the ideals of free-hand drawing. The object is to produce an accurate representation and anything which will aid in this is legitimate.

The size of drawings.—Probably every student confronted with the problem of preparing a drawing for reproduction is puzzled as to how large the drawing should be. It should be remembered that in making the reproduction the figure may be reduced any amount that is desired or it may even be enlarged. Usually it is reduced in size, for it is generally not practical to make the original as small as the reproduction will be. Then too a reasonable degree of reduction aids in smoothing out minor roughnesses of line.

The general tendency is to make drawings too small. Ideally the reproduction should be large enough to permit its ready use. It should be of such a size that everything can be seen easily and with a minimum of study; consequently the size should be regulated by the smallest included object that is to be seen easily. If the details must be shown in a larger size they can be included in special drawings. On the other hand, to make the reproduction too large is to waste space, to increase the cost unnecessarily, and to give an appearance of crudeness.

It is always easier to work with a large drawing than with a small one. In a small drawing a very slight error will be very evident and the mere width of a line may be important; while in a large drawing a slight deviation is unimportant. Then too it is easier to draw large. The figures accompanying this chapter have been reduced from one-half to two-thirds.

The preliminary drawings.—If we are to draw a microscopic object with the aid of the camera lucida[2] we shall always meet with certain practical difficulties. No matter how carefully the specimen has been prepared, it is almost certain to be imperfect and to some degree it is necessary to undertake a reconstruction.

With the aid of the camera lucida preliminary sketches are made on the thin copy paper. Usually the object will be so large that it will be impossible to get it all into the field at once and it must be drawn in sections. If care be taken to see that each section has two or three points in common with each adjoining section they can be pieced together easily. It is not necessary to indicate every detail on these drawings; all that are needed are the outlines and the more important points of reference.

Usually, if the object is transparent, it is not necessary to draw both halves of the body. All the structures, dorsal and ventral, can be located on one half and later separated out. The object will probably be distorted, the head perhaps bent one way, the thorax another, the abdomen another and the legs variously disposed. All these things can be straightened up later.

Having gotten into these sketches everything that will be necessary, they can be corrected and smoothed up and then transferred to the drawing paper. To do this simply take a soft pencil and follow the lines as they show through on the back side of the tracing paper. Then by placing the copy paper upon the drawing paper and following the lines with a hard pencil a carbon copy will be produced.

To straighten up a distorted specimen, the following procedure may be used. Draw on your sketch a center line for each part of the body. Draw on your drawing paper a line representing the median line of the body. Place the sketch upon this, with the center line of the head coinciding with the center line on the drawing paper. Trace the head. Pivot the sketch about what appears to be the natural pivoting-point until the center line of the thorax in turn coincides with the center line on the drawing paper. Trace the thorax. Proceed in the same way with the abdomen. If care has been taken in selecting the pivoting points the whole object will be straightened up.

The same method can be applied to the arrangement of the legs, which are almost invariably badly disposed in the preparation. Note that the segments of the legs pivot at definite points. Transfer the femur, let us say, and pivot the drawing upon the pivoting-point of the femoral-tibial joint until it assumes the desired position; and so on.

[2] This indispensable aid to drawing will be found described in any book dealing with the use of the microscope and its accessories.

The writer wishes here to emphasize a certain point of view. The drawing is intended to represent the object divested of all extraneous factors. The fact that the individual specimen went through certain vicissitudes in the course of its preparation is such a factor. Consequently there is no valid reason for perpetuating those vicissitudes in the drawing. If at the last moment in making the preparation a vagrant current of balsam twisted the legs about, there would seem to be no special reason for perpetuating that mischance forever in print. To do so is simply a sort of pseudo-accuracy—or it is plain laziness. Some type position should be adopted, a position that is as natural as the preparations will permit, and all drawings of specimens belonging to the same group should conform to that position.

With one side of the body transferred to the drawing paper in proper form it is usually desirable to retrace this finished drawing, as far as the outline is concerned, and then, turning the tracing over, transfer it for the other half of the body.

With the outlines worked in it is then possible to arrange the details. The degree of care necessary in making the pencil sketch depends upon the skill of the illustrator. It may be desirable to draw absolutely everything in pencil first of all, or it may be possible merely to draw the major features and then to work the details in directly in ink.

One thing is to be remembered. *Never cease studying the object until the drawing is finished.*

Divided drawings.—The work of producing drawings in conformity with the ideals which we have expressed is by no means slight. It is laborious, eye-straining, and time-consuming. Furthermore, the cost of publishing the essential figures is an item that must be seriously considered. While it is the writer's contention that none of these difficulties should be allowed to interfere with the development of our work, it is nevertheless entirely legitimate to accept any compromises that do not stultify our ideals.

One of the most practical compromises is that of the divided drawing which shows one half of the dorsal and one half of the ventral side of the insect. Such drawings are made possible by the fact that insects are at least theoretically bilaterally symmetrical objects. Actually they are probably never so, for there are variations even as between the two corresponding halves of the body, and in a few cases there is a genuine secondary asymmetry. Nevertheless the departures from the theoretical condition are so slight that they do not at all preclude the use of these drawings. Cases of actual asymmetry are so few that they can be specially dealt with.

So if the right and left halves are simply mirror images of each other, all the essential requirements are fulfilled if only one half be figured. Just when and by whom this method was originated the writer cannot say. It occasionally appears in one form or another in various places throughout even some of the older entomological literature, but it is only in recent years that it has come to be extensively used. It has been employed especially by students of the Coccidae, a group in which its advantages are most obvious, but it has been utilized in numerous other groups as well. The author has employed it in the Diptera Pupipara, Mallophaga, Anoplura, Coccidae, Aphidae, and Chermidae. Its advantages are so great and its disadvantages so few that it should be accepted generally as a standard method of illustration throughout the arthropods.

It is of interest to note that exactly the same method is employed by marine designers. A boat is a bilaterally symmetrical object, thus permitting the use of such drawings, and they are standard practice in boat designing.

Certain objections, which the writer regards as quite unimportant, have been urged against these drawings. There is the objection that the drawings are not "artistic." As has been pointed out, we are concerned with drafting, not with Art. Neither is an architect's blue print nor a topographical map artistic in such a sense. It has been urged that structures along the median line are obscured. This has some slight validity, in such cases for example as the presence of a median longitudinal furrow; but on the whole it is insignificant. It has been urged that the drawings "look peculiar," an objection that can hardly carry much weight.

The saving of labor in producing the drawings is a great advantage. So is the saving in cost of reproduction, for obviously to present complete figures of both sides of the insect would double the cost. Yet under the standards which we have proposed the complete figuring of both sides is demanded in many cases. These advantages alone seem sufficient to offset any disadvantages and to justify the drawings.

Various suggestions have been made as to the exact form that such drawings should take. These are illustrated in the accompanying diagrams. It has been suggested that the median line be prolonged past the extremities of the figure (Fig. 1B, p. 86); that the two halves be slightly separated (Fig. 1A); but the writer inclines to the use of the form shown in Fig. 1C, in which the two halves are not separated and the median line is not produced. This has the advantage of showing the outline of the body undistorted, and there seems to be no gain in the produced median line.

Whether the dorsal side should be to the right or the left is entirely a matter of choice.

In drawing winged forms it will usually be advisable to draw the wings separately.

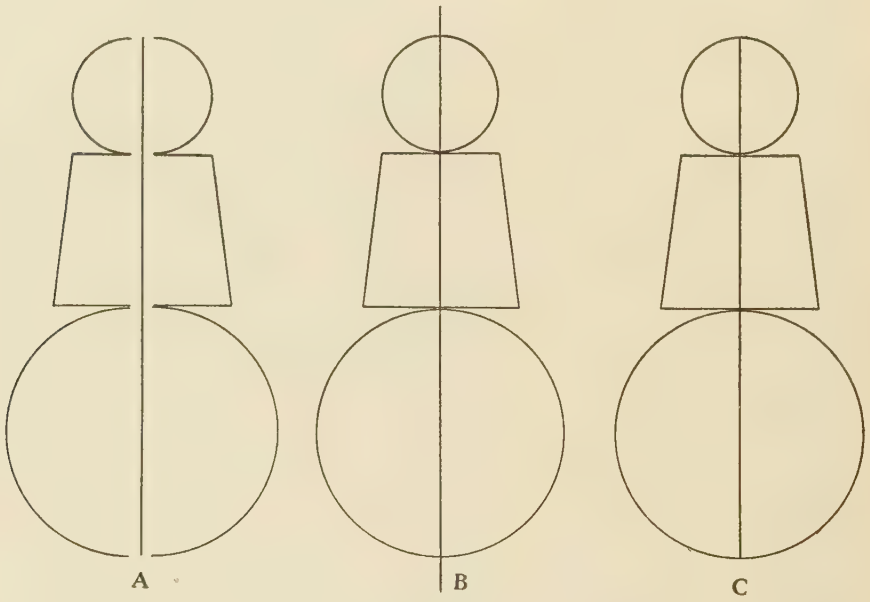

FIG. 1.—Diagrammatic representation of different methods of employing divided drawings for bilaterally symmetrical objects.

Outline drawings.—The virtues of simplicity are frequently overlooked. Consequently there is a very strong tendency among illustrators to produce drawings that are unduly elaborate, and the possibilities of the simple outline drawing are commonly not recognized. Yet, in all probability, about ninety per cent of all the work that the average entomological illustrator has to do can be presented in the form of plain outline drawings or some very slight modification of them.

There are, of course, occasions when it is desirable to represent such things as markings or color areas, and these demand a more elaborate technique. Nevertheless, if one deliberately set himself to develop the outline drawing to its highest limits he will find that there are surprisingly few cases where it is not adequate, especially if supplemented by notes.

It is quite true that the outline drawing in its simplest form is by no means esthetically pleasing, especially if it be badly made. There are a number of simple things which can be done to such drawings to increase their attractiveness and relieve them from the monotony of dead flatness, without at all departing too widely from the ideal of simplicity and clarity.

When such things can be done it would seem quite reasonable to make use of them. Their application will be discussed later.

Lines.—The basis of pen and ink work is a good line, and a good line cannot be made without practice. It should be firm and even, never scratchy. Care must be taken, because of the practical requirements of reproduction, that the line shall not be too fine; otherwise it may break down. On the other hand, the lines should not be coarse and crude.

The way to secure such lines will vary with the individual. Whether the pen is moved away from the draftsman or toward him, just how it is held, the position of the arm—all these are things for the draftsman to determine for himself by experiment and study of his individual requirements.

The engineering draftsman utilizes the ruling pen in its various forms almost exclusively. The entomological draftsman might do this also were it not for certain practical difficulties. An ordinary entomological drawing will involve a great many curves of varying characteristics, these passing insensibly one into another. The draftsman can, if he desires, fit curves to these lines; but the time involved in doing so will prove to be excessive. Consequently it will usually be more practical to develop the ability to draw a smooth, even line freehand.

Shading.—In such drawings as these, which are purely conventionalized representations, we are not concerned with trying to give a "lifelike" appearance. But there are occasions when a bit of shading will aid in making clearer the things that we wish to show, and in any drawing the deadness and flatness of pure outline can be relieved by its use.

The simplest method of shading is one that is commonly employed by engineering draftsmen. Let us assume two small areas of the same shape, one an elevation, the other a depression. In a simple outline drawing the distinction will not appear. To show the difference, let us assume that the light comes from the upper left-hand corner of the drawing at an angle of forty-five degrees, and that the rays are parallel. In the case of the elevated area the shadow would be cast at the lower right-hand side, and in the case of the depression it would be at the upper and inner left-hand portion. By increasing the thickness of the lines on these sides an indication of shading can be given.

Fig. 2 (p. 88) is intended to illustrate this point. The impression of elevated and depressed areas is clearly given by this simple conventionality, as can be seen upon comparison with the lower drawing, in which this is not attempted. By the consistent application of this principle the relations of the various planes in even a rather complex object can be clearly shown. The excessive labor and the necessity of developing an elaborate

technique that would be involved in the preparation of a completely shaded drawing are thereby avoided. Furthermore the attempt to shade a drawing completely frequently results in nothing more than the obscuring of many things that it is desirable to show.

Examples of the application of this principle to entomological drawings will be found in all the figures that illustrate this chapter. It may be noted that if the simple broadening of the lines be regarded as pro-

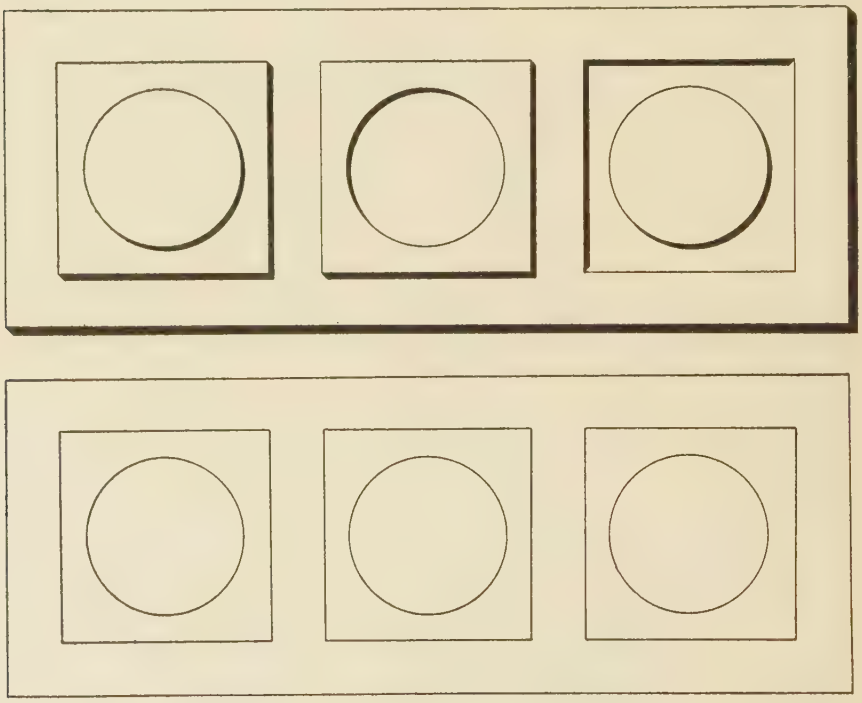

FIG. 2.—Diagrams showing the effect of conventionalized shading as employed by mechanical draftsmen.

ducing a drawing that is excessively harsh the effect can be softened by a row of stipple following the shaded line.

The student who is making his first illustrations is usually much attracted to the use of some form of stipple in his figures. This consists of the application of the ink in dots. Many illustrations can be pointed out, even in standard textbooks, where this method has been employed, frequently for no apparent reason. There are many occasions where it is useful in simple drawings. It is particularly so in the making of

morphological drawings, where stipple is conventionally employed to indicate membranous areas.

Properly done this is quite effective, but it is usually not done properly. Ordinarily the dots are made with a pen not adapted to such work and are of all sizes and shapes and irregularly arranged (Fig. 3A). Frequently, if reproduced, many of the dots will disappear, having been too lightly made, and the reproduction will be blotchy. Good stipple is difficult to produce. Ideally the dots should all be perfectly round, of the same size, placed in little circles, not in rows, and evenly spaced. The example here presented (Fig. 3B) for comparison can by no means claim to be

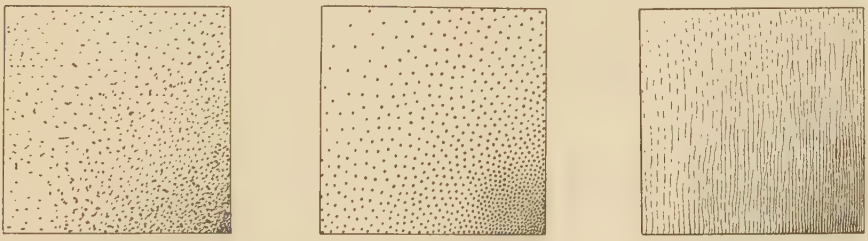

FIG. 3.—Simple types of shading. *A*—pseudo-stipple; *B*—stipple; *C*—lines.

technically perfect. The method is so extremely slow that its use by the average worker will hardly be justified except in special cases.

Although the use of lines to give different surface effects and to indicate shading is much quicker than stipple it is more difficult to master. For the kind of drafting which we are here considering there is but little need of such work and but a few of the infinite variety of types need be considered.

There are occasions when some pigmented or sclerotic surface can be made more evident by the use of such lines as indicated in Fig. 3C. Ordinarily more or less parallel lines, broken and slightly irregular at intervals to keep the whole from appearing too hard and harsh, will be sufficient.

Spines and setae.—When the derm of almost any insect is examined closely it will be found to bear a larger or smaller number of spines or setae of various sizes and shapes. In many cases these are quite small, and except in certain groups relatively little attention has been paid to them by systematists for the prevailing methods of preparing material make it impossible to see them clearly. But with adequate preparations they become very evident and the student who works with such material quickly becomes impressed with their importance.

The careful recording and representation of such objects as regards size, shape, numbers, and distribution is extremely important. The degree of care in counting setae varies inversely with the number. Obviously, if but two setae are present a variation of one is a variation of fifty per cent; while if five hundred are present an error of as many as twenty-five is relatively unimportant, for it probably is no greater than the normal range of variation. It follows as a rather definite rule that the smaller the number the more carefully should it be determined. Where the number is very large a sufficiently accurate estimate can usually be arrived at without an actual count.

In the drawing of such setae there are several points to be observed. Note that all setae arise from a socket, which appears as a ring about the base. A much more finished appearance is given to the drawing if this socket is indicated. Compare the two portions of Fig. 4B. Note

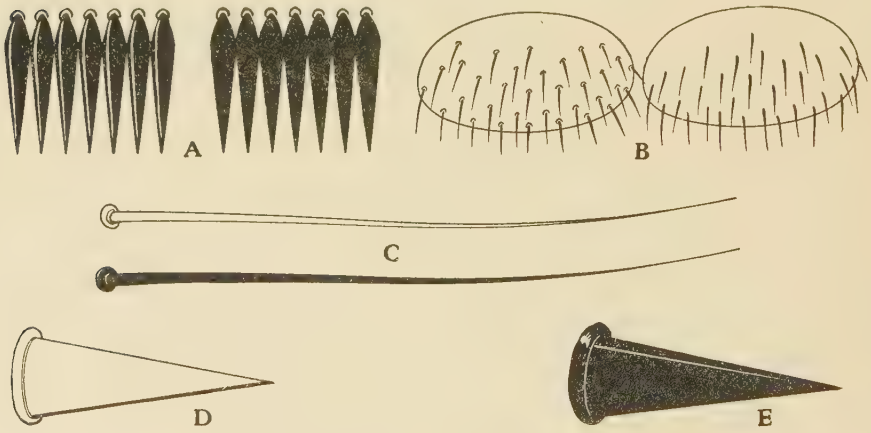

Fig. 4.—Methods of drawing setae.

also that setae usually have some quite definite shape and this should be represented carefully whenever the size of the drawing permits.

There are two methods that may be employed in drawing setae. One is that where the outlines merely are shown; the other, where the setae are indicated in solid black (Figs. 4C, D, E). The former is the more pleasing in appearance, but it is the writer's belief that the latter impresses itself more readily upon the eye; and the latter has the advantage of being easier to draw for irregularities are not so conspicuous. In representing large setae by the second method it is possible to give an impression of their form by the use of a conventional high light as in Figs. 4A and 4E.

Sclerotic areas.[3]—The question of relative degree of pigmentation of sclerotic areas is likely to cause some trouble. On the one hand, if we attempt to show by stipple or line these differences we immediately get into difficulty because we entirely obscure everything else. This difficulty is so serious that the writer contends no attempt should be made to do anything of the sort except in special cases. For example, if we refer to Fig. 5 (p. 92) we can see what would happen. In this insect the entire body, except for membranous areas on the abdomen and lateral regions of the thorax, is quite heavily pigmented. To attempt to show the degree of pigmentation would simply result in the complete obscuring of the vestiture of setae. Nor would there be any compensating gain, for the actual degree of pigmentation is not at all important in the study of the group to which this insect belongs. It can be indicated sufficiently well by words.

On the other hand, there are occasions when the effectiveness of a drawing may be greatly increased by attempting to indicate the degree of pigmentation. Fig. 6B (p. 93) represents a species of the Mallophaga which belongs to a group in which the various dark markings are important specific characters. Here the vestiture of setae is simple and relatively unimportant. Consequently it appears reasonable to attempt to indicate the pigmented areas by something more than mere outline. In Fig. 6A, which is of another species of Mallophaga belonging to a different group, the relative degrees of pigmentation are again unimportant and it is sufficient merely to indicate the form and extent of these areas by outline.

It will be found that in the vast majority of cases the latter condition will rule, and before attempting anything more than this the question of whether anything at all is to be gained should be considered very carefully. Usually to go any farther is simply to ask for trouble.

It is quite true that in drawings where sclerites are indicated by enclosing lines we sometimes get inconsistencies. Suppose we have a sclerite enclosed within a membranous area and in turn enclosing such an area. There is no adequate way of indicating this merely by outline. Also there are inconsistencies where sclerites of varying degrees of pigmentation are indicated by lines of the same character. This difficulty, however, can be partially obviated by the use of broken lines, that give an impression of weakness, for the areas which are weakly developed.

These difficulties give little trouble, however, to one who is accustomed

[3] Attention should be called to the use here of the word "sclerotic" in place of the common usage, "chitinized." This is in accord with conclusions expressed elsewhere. See G. F. Ferris and J. C. Chamberlain, "On the Use of the Word 'Chitinized'," *Entomological News,* XXXIX (1928), 212–215.

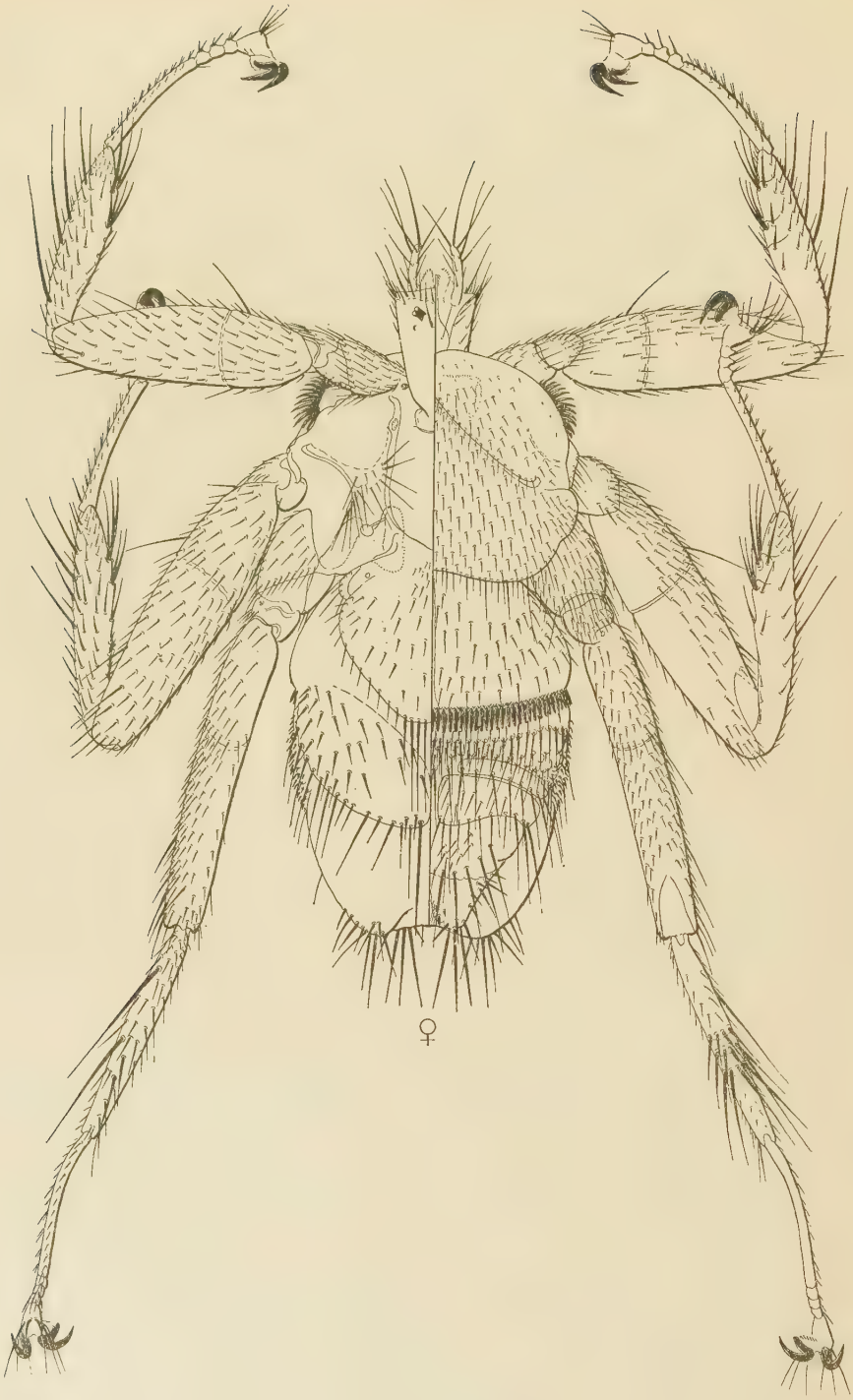

FIG. 5.—A Nycteribiid, *Basilia antrozoi* (Townsend), illustrating the application of the methods herein described to the figuring of a flattened, but rather complex, object.

to the use of drawings of this type. The drawings are frankly conventionalizations to a very considerable degree and one simply becomes accustomed to accepting and interpreting them.

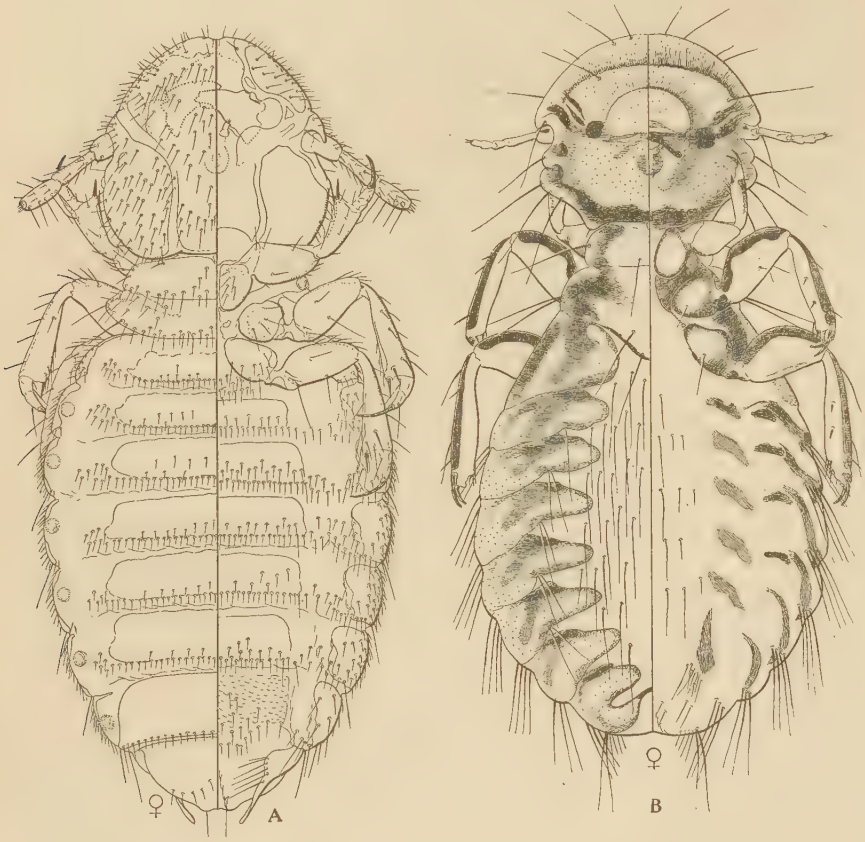

FIG. 6.—*A*—A Mallophagan, *Trichodectes bovis* (Linn.), illustrating the use of pure outline. *B*—A Mallophagan, *Goniocotes gigas* Taschenberg, illustrating the use of a simple type of shading where it is desirable to indicate degrees of pigmentation.

Internal structures.—Where it is desired to show a structure that is within or beneath another structure, the conventional method is to represent the concealed portion by dotted lines.

The preparation of figures for publication.—If figures are to be published care should be taken in their preparation. Usually they will be made upon a drawing paper that has not the requisite stiffness to lie flat, and in addition they will need to be properly arranged on the page. They can of course be drawn upon a single sheet in the arrangement that is to

be final; but ordinarily this is impractical, and the better method is to cut them out of the drawing paper and arrange them upon a sheet of rather stiff cardboard to which they are then glued. It is recommended that this cardboard be white, as sometimes the shadows of the pieces of drawing paper will photograph as a line. The figures should be carefully arranged. On the one hand, they should not be so widely spaced as to waste space; and, on the other, they should not be so crowded that their usefulness is impaired.

Frequently much experimenting is necessary in order to arrive at a practical and pleasing arrangement of the figures. All of this is worthy of consideration, for one very frequently sees plates of drawings, the effect of which is greatly diminished by bad arrangement. Detail drawings may be grouped about the larger figures, and frequently it is advantageous to connect the details with the corresponding point in the larger drawing by means of a ruled, dotted guide line.

The lettering or numbering of the figures should also be done with care. Ordinarily it will save time and produce a much better appearance to have printed letters that can be pasted on the drawing. The letters or figures should be so arranged with reference to the drawings that there can be no possible doubt as to which they refer. Also they should be arranged in such a sequence that any given figure can be found easily. It is most annoying to see a page crowded with small figures and with number one at the top alongside perhaps of number thirty, and number two at the bottom in company with number twenty. The letters or numbers should be large enough to be easily read, but not so large as to attract more attention than do the drawings.

It is recommended, and some journals request, that if a page of drawings includes figures representing more than one species the name of the species should be written under the drawings referring to it. This will frequently aid greatly in the use of the figures. The captions for figures should be full and complete and should be carefully checked over to see that they agree with the figures.

In mailing such figures they should always be mailed flat and they should be protected against breakage by a sheet of carton paper or very stiff cardboard.

Explanation of certain of the accompanying figures.—The figures accompanying this chapter have been selected in order to illustrate some of the various problems which the student will encounter and to answer certain criticisms that may possibly be directed at some of the statements that have been made in the course of this discussion.

Figure 7 represents the pygidium of a scale insect, *Chionaspis salicis*

(Linnaeus). Insects of this sort are relatively simple things and the attainment of the ideal of figuring every structure that is to be seen upon the body is comparatively easy. For our present purposes it is sufficient to show merely the portion that is here figured. Such a drawing, however, needs to be supplemented by a general drawing of the entire body and by numerous detail figures.

The pygidium of scale insects of this type is usually more or less sclerotic on the dorsal side, and the surface is marked with minute ridges, resembling very much the ridges of the finger tips. This condition being common to all the species of the group, with only occasional exceptions,

FIG. 7.—Slight modifications of the outline drawing as applied to the pygidium of a scale insect, *Chionaspis salicis* (Linn.).

it would seem unnecessary to attempt to show such striations completely in the figure. The writer has therefore adopted the conventionality of merely indicating these markings at the margins of the areas which they cover. The tubular ducts on the dorsal side are internal structures and consequently are shown by dotted lines. Such structures should be shown on the side from which they originate and there would seem to be no point gained by indicating them from the other side as well. The logical method would seem to be to regard the dorsal and ventral sides as absolutely independent of each other, separated as it were by an opaque wall, even though in transparent preparations both sides may be visible at once.

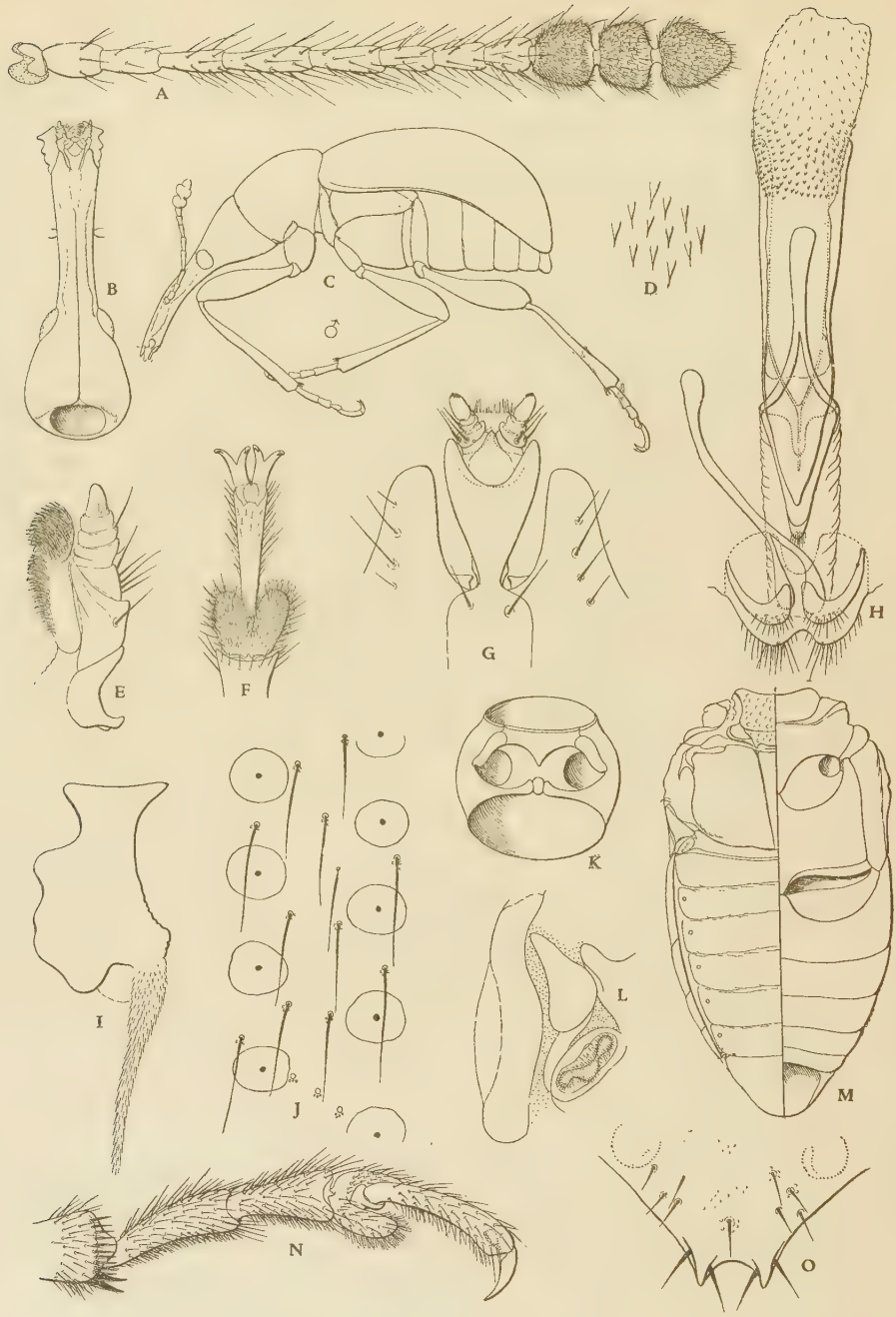

FIG. 8.—Application of the methods herein described to a rotund and complex object, a beetle of the genus *Rhynchites*. *A*—antenna; *B*—ventral aspect of head; *C*—lateral aspect of entire insect; *D*—ornamentation of ventral side of elytra; *E*—maxilla, ventral aspect; *F*—plantar surface of terminal tarsal segments; *G*—ventral aspect of labium; *H*—genitalia of male; *I*—mandible; *J*—ornamentation of portion of dorsal surface of elytra; *K*—ventral aspect of prothorax; *L*—region about first abdominal spiracle; *M*—meso and metathorax and abdomen; *N*—posterior tarsus; *O*—tip of rostrum, dorsal aspect.

In this drawing the slight indication of shading along the margin aids in expressing the various degrees of pigmentation of the structures and eliminates the dead flatness which would otherwise result.

In Fig. 8 is illustrated the application of these methods to a beetle of the genus *Rhynchites*. In the case of a form such as this the problem is a bit more difficult than it is in the case of a very flat object such as the scale insect of Figure 7. There is required a drawing, such as Fig. 8C, in which the lateral structures also can be shown. It may be noted in connection with these figures that the writer has apparently disregarded his own principles. This insect is quite thickly and uniformly beset with a vestiture of short setae and these setae are not indicated in Figures 8B, 8C, or 8M. This apparent discrepancy is deliberate and results from purely practical reasons. If any attempt were made actually to represent in these figures this coating of hairs all the other structures would be either greatly or entirely obscured. The vestiture of setae being very uniform it is sufficient to call attention to it in the written description and to indicate something of its detailed nature as in Fig. 8J, which shows a small area from one of the elytra.

Figure 8B, which represents the under side of the head, illustrates one of the minor disadvantages of the divided drawing. There is here a median furrow, representing the invaginated gular plate, and this furrow would coincide with the line dividing the two halves of the drawing. Consequently, the divided drawing must here be discarded.

The student of this group might very well find details, in addition to those shown here, which he would wish to figure. It might be desirable, for example, to figure the dorsal side of the head, the inner face of the labium or the maxilla, or the opposite face of the mandible, cross-sections of various portions, and so on. It is in the possibility of such things that the judgment of the student is called upon. It is quite evident that in the figures here presented the ideal of recording *absolutely everything* to be seen upon the body of the insect has not been attained. Nevertheless, it is evident that the ideal can be so closely approached that the failure is by no means depressing.

The drawing of the wing of this beetle in Fig. 9 (p. 98) demands a somewhat more elaborate technique, chiefly because of the varying degrees of sclerotization of the veins, but it is really quite simple. The labor of indicating the minute setulae with which the wing is beset is time-consuming, but is not at all technically difficult.

In Fig. 10 (p. 99) are shown certain drawings selected to represent the simpler work of one of the most finished entomological illustrators of the present day. This illustrator is one who combines the qualifications of

both the artist and the scientific investigator to such a degree that his illustrations are not only pleasing to the eye but technically impeccable as well. The writer would recommend that the student who is interested

FIG. 9.—Wing of beetle of genus *Rhynchites*.

in the subject of entomological illustration should study this work of Mr. A. J. E. Terzi. It appears especially in the volumes of the *Bulletin of Entomological Research*. The student will find no better teacher.

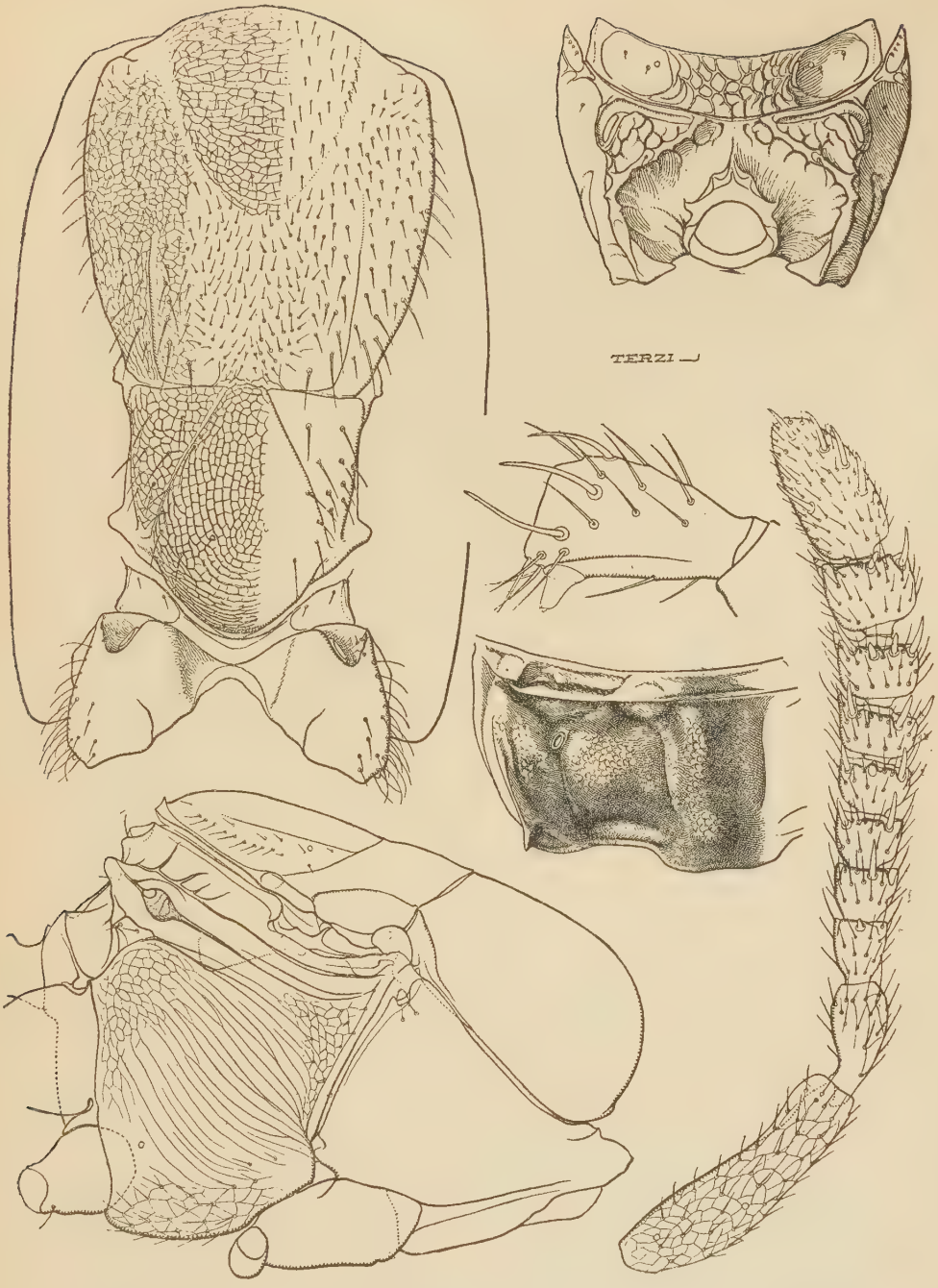

FIG. 10.—Selection of drawings illustrating technique of a skilled entomological illustrator. Note the methods by which the surface ornamentation is indicated.

CHAPTER IX

THE DESCRIPTION OF SPECIES

The mere recording of the data, however well it may be done, does not conclude the work of the systematist. It is necessary also that these data be analyzed and their relative importance and significance determined as far as may be in order to facilitate their future use. This is the principle function of the verbal description.

We have said that words are an unsuitable tool with which to attempt the complete recording of the data upon which the recognition of species—in entomology at least—must for the most part rely. They have, nevertheless, an indispensable place in the work of the systematist. However completely and accurately the record may have been made by the methods of the draftsman, there still remains a function for the verbal description.

For one thing, it is not practically possible to tell the whole story with the help of figures alone. There are certain data, the recording of which must be left to words because of practical considerations. Such data are those concerning variation, distribution, ecology, habits, information obtained from experiment and the like. Such information should be included as a part of the verbal description.

But this is not all. With all of its advantages in the making of a precise and definite record, the method of figures cannot do everything. We cannot, because of the severely practical limitations of cost, figure every insect in color. Then too there are some things that can be even more easily described by words than they can by figures. If we have, for example, a large surface uniformly covered with setae or some other structures it may be far more simple and just as easily understandable to describe this by words. These are all practical considerations which will be readily appreciated.

There is also one formal function of the verbal description which, under the existing codes of nomenclature, cannot be ignored. It is quite generally held that to be valid any new name must be proposed in company with a verbal description and in some codes of nomenclature this is specifically asserted. A recently proposed entomological code has indicated this as a definite rule from which the Lepidoptera, for some mysterious reason, are alone excepted. Although the insistence upon this point probably arises from reasons having nothing to do with the relative merits of words and figures in characterizing an object, the rule might very well be insisted upon even by one who believes that the verbal characterization which is admitted as a legal description under all codes is frequently nothing short of farcical.

It should be possible to assume that the individual who has given to any organism the amount of study necessary to qualify him to pronounce as to its specific distinctness and as to whether it is heretofore unrecognized is qualified to express some conclusions concerning it. It is to be expected that he will have some ideas as to the relative significance of the data which he has accumulated. For him to present merely the bare data without any analysis of them and without any attempt to weigh and to sift them is to indicate either that the author is incompetent to form any conclusions or that he has been content to perform but a portion of his task.

It is quite evident that of the entire range of structures presented by a species, some will be more important than others in the practical tasks of later identification and in any attempt to place that species in a classificatory system. As we have already pointed out, it was the recognition of this fact which made possible and gave justification to the use of the specific diagnosis. This diagnosis, separated, as it has usually been, from the data themselves, has proved a most inadequate instrument, for there was no method, short of a re-examination of the material upon which it was based, by which it could be checked and its validity determined. On the other hand, the writer would contend that the formulation of what is in effect this same diagnosis, taken together with the data from which it is drawn, is the essential part of the function of the verbal description. For the describer of a species to present merely the raw data is comparable to the action of a cook who would place upon the table merely the raw materials that are to be the ingredients of a meal. To do so is to impose upon the user of these materials a task that is not properly his.

Ideally, it is the describer who should give to a species the maximum of study in order that the results of his work of segregating the species from its fellows and the information obtained thereby shall be immediately available.

The description then should include as a basic function the presentation of an analysis of the available data. It should serve to call to the attention of the user those things which appear to the describer, in the light of his presumably special knowledge, to be of significance. Here, it would seem, we have again exactly the same principle which has governed the specific description in the past. It would appear that the writer is inconsistent, that he is reverting to the very method which he has previously condemned. He has once more admitted the variable judgment of the describer to complete respectability! But after all, there is a very significant difference.

It may very well be that, in spite of his presumably special knowledge, the judgment of the describer is at fault. He is still confronted by

certain of the difficulties which we have previously discussed. It may very well be that further work, the discovery of more forms and the increase of knowledge, will show his judgment to be deficient. Cértainly his conclusions will need to be tested by further work. But the essential point is this: *If his work of recording the data has been properly done those data are available for re-examination and revaluation. His conclusions can be checked, they can be extended or modified or rejected as appears desirable, all without the necessity of recourse to his types. But the presentation of his conclusions is essential as the first step in the evaluation of his data.*

Under such a system then, the specific description, in addition to including those data which we have discussed, will consist largely of what is in effect an abstract of the important features revealed by the figures, leaving the details to be obtained from the figures themselves. The description will not in any way attempt to duplicate the figures, any more than the abstract of a paper attempts to duplicate the paper itself.

There are certain practical considerations to which it would be well to refer. The writer would not be interpreted as holding that absolutely every species should be figured completely. If we have, let us say, a genus of ten or so species all of which differ from each other only in certain details, the reasonable thing to do would seem to be to figure one species completely and then to describe the others by reference to this, noting merely the points of difference. This indicates the great desirability of standard figures, representing wherever possible the types of genera especially. Forms differing to such an extent that they cannot well be described adequately by such a system should themselves be figured.

In connection with the describing of species there are certain formal requirements that should be observed. The editors of the *Proceedings of the Entomological Society of Washington* have proposed a set of rules to cover the form of papers which are to be published in that journal.[1] These rules are entirely reasonable and their scrupulous observance by all students would go far toward increasing the usability of systematic work.

The rules and suggestions are as follows:

Rule 1. No description of a new genus or sub-genus will be published unless there is cited as the genotype a species which is established in accordance with current practice of zoölogical nomenclature.

Rule 2. In all cases a new genus, or sub-genus, must be characterized, and if it is based on an undescribed species the two must be characterized separately.

Rule 3. No description of a species, subspecies, variety, or form will be published unless it is accompanied by a statement which includes the following

[1] *Proceedings, Entomological Society of Washington,* XX (1), 1, (1918).

information when known: (1) the type locality; (2) of what the type material consists, with statement of sex, full data on localities, dates, collectors, etc.; and (3) present location of type material.

Rule 4. No unsigned articles or articles signed by pseudonyms or initials will be published.

Rule 5. The ordinal position of the group treated in any paper must be clearly given in the title or in parentheses following the title.

Suggestion 1. All illustrations, accompanying articles, should be mentioned in the text and preferably in places where the object figured is discussed.

Suggestion 2. It is desirable in describing new genera and species that their taxonomic relationship be discussed, and that distinguishing characters be pointed out.

This second suggestion is worthy of some discussion, for it touches upon an important point. Concerning this, the writer desires to quote the remarks of Harrison, which apply quite forcefully:

> First I would wish to insist on the discussion of a new species with reference to its nearest relatives, *if these can be accurately determined.* When Giebel stated that his *Nirmus angulicollis,* from a petrel, found its nearest ally in *N. fenestratus* of the cuckoo, he wrote mischievous rubbish. The comparison of species by dumping them into artificially contrived groups may be almost as dangerous. Kellogg writes of *Lipeurus absitus* from the hoatzin: "This new Lipeurus of the group *clypeati sutura distincta* (all the other members of which have been taken from strictly maritime and coast birds) is thoroughly distinct from the other species of the group." This statement is, of course, absolutely correct; but the inference is that *Lipeurus absitus* is more closely related to these marine *Lipeuri* than to any others, which is certainly not the case. Obviously the group *clypeati sutura distincta* is not a natural one, or, at all events, its definition should be limited; and no good can come of comparing two unrelated species because they happen to fall within its artificial limits.[2]

With these views of Harrison the writer would concur entirely. In view of the condition of our systematic literature it is very frequently possible to say of a new species nothing more than that it is undescribed. The inability to indicate its affinities is not necessarily the fault of the describer nor necessarily an indication of a reprehensible lack of knowledge on his part. It may be the fault of earlier work, instead. To describe the new species accurately and completely, however, in spite of this deficiency may afford a point of entrance for a more thorough study of the other members of its group.

In the presentation of a specific description all the data should be arranged in some logical and orderly fashion. It would almost seem

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[2] Harrison, L. A., "The Genera and Species of Mallophaga," *Parasitology,* IX (1), 7, (1916).

unnecessary to insist upon such an elementary rule, and yet an examination of any considerable number of specific descriptions will show that it is frequently disregarded. A description which begins with the antennae, passes to the abdomen, back to the head, then to the wings and legs, is not at all uncommon.

The data may be arranged in various ways. One system, for example, would be to arrange characters in the order of their importance and such a system seems at times to be used. This may have an appearance of being logical, but it is likely to be difficult of use, for it will pass from one portion and one aspect of the body to another. A more usable system would appear to be to begin with a general statement as to size, color, and general characteristics, passing then to the characters of the head and its appendages, the thorax and its appendages, and thence to the abdomen, ending with the terminal segments. It might be well in the case of some forms to describe the dorsal and ventral sides separately. Where the sexes differ materially they should be described independently.

To increase its usableness a description should be properly paragraphed and the initial words of important paragraphs emphasized with italics or capitals. The supplementary data concerning localities, types, and the like should be treated in the same way. The following form will indicate some of the possibilities.

FEMALE (Fig. X). Length..... General color.....
.....
.....

Head..... *Antennae*..... *Mouthparts*.....
.....

Thorax..... *Legs*..... *Wings*.....
.....

Abdomen..... *Genital segments*.....
.....

MALE (Fig. Y). Length..... Similar [let us say] to the female in all respects except character of genital segments.

Genital segments (Fig. Y1).....
.....

TYPE LOCALITY .....

TYPES .....

NOTES .....

The consistent utilization of such a form, especially throughout the work of an individual author and throughout a single group, will aid greatly in the utilization of descriptions.

In connection with this discussion of the description of species the writer wishes to emphasize the importance of the *redescription* of inadequately described forms. A distressingly large percentage of the named species, in almost every group of the insects, cannot be recognized positively or even at all, on the basis of the existing literature. *It is more important, for the advancement of our study, to redescribe such forms than it is to describe new species.* The redescription of such forms should be regarded by the student as an essential part of his work upon any group which he may elect to study. The fact that a species has been *named* should make no essential difference in the way in which it is treated. As we have previously pointed out, the proper aim is not to *name* species but to *know* them. *The writer who contributes to the genuine knowledge of species is accomplishing far more than one who merely names them.* The fact that the author's name accompanies the names of the new species which he describes should not be allowed to influence his activities.

Keys.—The analysis of the data upon which specific descriptions are based is not complete with the mere writing of the description. To extract from a group of descriptions, however good they may be, those characters which will permit of the most ready identification of a series of forms, is laborious and time-consuming. Consequently, the work of identifying species is greatly facilitated by the use of tables or keys in which the salient characters have been tabulated. The making of such keys is nothing more than an extension of the process of analyzing the data, which begins with the specific description. Although such keys may be immediately concerned with families or genera or other groups higher than the species, their ultimate goal is in all cases that of specific identification. Consequently this would seem to be a logical place in which to discuss their development.

The making of such keys is a most important part of the work of the systematist. It is in such work that his grasp of his subject and his ability to make his knowledge available to others is most severely tested. That this should be so is easy to understand, for the making of such keys is nothing more than an additional test of the systematist's powers of analysis. An examination of many of the existing keys will show how great the failure to meet the test may sometimes be.

In the preparation of a key it should always be borne in mind that it is a purely practical thing intended for the use of those who know less about the subject than does the maker of the key. Its sole function is to

permit the most ready and the most positive identification of the forms with which it deals. Anything that interferes with this function should be rigidly excluded, and as far as possible the maker of the key should endeavor to place himself in the position of those who must later make use of the key.

Yet all too frequently we find keys that are practically useless. Consider the futility of a key to the Aphids of a certain political division which begins:

Winged females known
Winged females unknown.

Or of another which begins:

Size large
Size small.

Or of another that begins with the adult female, transfers to the male—which in the group in question can but rarely be secured—then passes to the first stage nymph and then back to the adult female again!

The characters used in a key should be as nearly as possible definite and positive. Of what value is a key which contrasts "five millimeters or more" with "five millimeters or less"? Yet such keys can be found. The alternatives should be clean cut and clearly stated.

Various forms of keys have at times been employed, and each particular type has its advocates. Let us consider some artificial examples:

I.

A. White.
  B. Front legs as long as the body.
    C. Antennae linear.
      D. Claws toothed ..... Species 1.
      D′. Claws not toothed ..... Species 2.
    C′. Antennae clavate .....
      D. Wings hyaline ..... Species 3.
      D′. Wings opaque ..... Species 4.
  B′. Front legs shorter than head and thorax.
    C. Eyes swollen ..... Species 5.
    C′. Eyes not at all swollen ..... Species 6.

A′. Black.
  B. Pronotum shiny.
    C. Anterior tibiae strongly curved ..... Species 7.
    C′. Anterior tibiae straight ..... Species 8.
  B′. Pronotum dull black.
    C. Posterior tibiae with apical spur ..... Species 9.
    C′. Posterior tibiae without apical spur ..... Species 10.

This form of key has many disadvantages. In the first place it is extremely wasteful of space. Keys can be found in which the indenting of lines has been carried so far that there is room for but one or two words on the shorter lines. Another objection is that the alternatives may, in a long key, be separated by several pages, making it necessary to hunt for some time to find them. It has been argued that such a key makes the relationship of the divisions more easily apparent to the eye, but even so this does not offset the disadvantages.

This type may be varied by doubling the letters thus, AA, instead of using A and A′.

II.

1. White 2
   Black 7
2 (1). Front legs as long as the body 3
   Front legs shorter than head and thorax 6
3 (2). Antennae linear 4
   Antennae clavate 5
4 (3). Claws toothed ..... Species 1.
   Claws not toothed ..... Species 2.
5 (3). Wings hyaline ..... Species 3.
   Wings opaque ..... Species 4.
6 (2). Eyes swollen ..... Species 5.
   Eyes not at all swollen ..... Species 6.
7 (1). Pronotum shiny 8
   Pronotum dull black 9
8 (7). Anterior tibiae strongly curved ..... Species 7.
   Anterior tibiae straight ..... Species 8.
9 (7). Posterior tibiae with apical spur ..... Species 9.
   Posterior tibiae without apical spur ..... Species 10.

This is in many respects the best type of key. The alternatives are side by side for ready comparison; in a long key it saves space; it can be run backward as easily as forward. It is not commonly employed, however, as here shown, for ordinarily the figures in parentheses by which the backward operation of the key is facilitated are omitted.

The ideal key should admit of the identification of a single specimen, assuming that specimen to be adequate for study, without reference to other specimens, to the opposite sex or to developmental stages. A key which makes use of averages is practically useless, for in almost all cases we must deal with individuals and not with averages.

If there is a marked sexual dimorphism a key should not pass from one sex to the other. Separate keys for the sexes should be arranged. If immature forms are to be dealt with they also should have a separate key.

CHAPTER X

CLASSIFICATION

The data accumulated in the process of segregating and describing species are the basis for the development of a classificatory system. It is the aim of such a system to chart the course of evolution, to express the varying degrees of relationship of the organisms involved. The essence of such a system is the arrangement of the organisms into natural groups of varying rank. The controlling standard is that of revealing in the clearest and most practical way the conclusions that have been derived from the preceding steps.

The describing of species is but a portion of the work of the systematist. It is in a way merely the assembling of the raw data. After these data are available there still remains the task of organizing them into a system which will display to the best advantage the information that has been gathered and will facilitate to the highest degree the utilization and the understanding of that information.

There are those who maintain that these two problems—that of describing species and that of arranging them into a system—are not properly to be undertaken by the same individual. They believe that the describing of species demands certain qualifications which are not necessary or are even undesirable in the accomplishment of the next step. The describer, they would say, is primarily a student of detail. The classifier needs the broad view, the ability to shake off the weight of detail and to select from the mass of evidence those things which are alone essential to his purpose.

It might in fact be possible to develop a rather strong argument in favor of such a division of labor if we consider merely the method of work and not the end to be achieved. Certainly the describing of species deals only with concrete things. One at this point is interested merely in accumulating and exposing in the clearest possible way the evidence that is to be found in the specimens before him. It is a concrete task, demanding nothing beyond the ability to see clearly and to display clearly the bare facts.

Classification is a very different thing. It deals only with concepts. It is purely subjective. A genus, a family, an order, these are purely taxonomists' ideas as to how the aggregates called species—whether the species be likewise purely a concept or not—may be grouped together in such a fashion as best to display their degrees of relationship. With a series of forms before him the systematist analyzes the data which they

reveal and draws from these data certain conclusions which he then endeavors to express by a system of categories of varying rank.

Nevertheless, it is the writer's view that the two functions should be combined within the same individual. For the describer of species to refrain from drawing any conclusions concerning those species is to reduce him to the level of a mere day-laborer. And, on the other hand, the taxonomist who attempts to deal only with abstract conceptions may easily find himself in the way of losing his grip upon reality.

Because of this circumstance that the work of classification deals only with concepts, it is a stubborn thing to reduce to any sort of formula. It is difficult enough to arrive at any satisfactory definition of the species, and yet there is at least some shadow of concreteness to this category. How much more difficult is it, then, to define the larger categories which are purely concepts, varying in time and with the mental makeup of the individual worker.

If one follows the course of the development of classification it is possible to see how clearly this difficulty appears. To Linnaeus a genus was a group of species of very broad limits. It was in fact equivalent to what is now regarded at least as a family, if not even a super-family or an order. To some modern workers it becomes reduced almost to a single species with its sub-species and its variants. In fact there are some who will argue that the genus is doomed eventually to disappear. There are others who, disturbed by the continual narrowing of generic limits will voice a "plea for the genus" or who will argue for a "return to Linnean genera." Between these extremes there is every possible variation.

The same conditions hold in the case of the family. This group is an aggregation of genera which are presumably closely related. But at what point should one family begin and another end? There are those who believe in keeping family limits extremely broad and others would limit them strictly and greatly increase their numbers. Who can say which proceeding is right and which is wrong?

Before we attempt even to approach a consideration of the problem it were well to get clearly in mind the function of a classification.

There is of course the very simple function of merely arriving at some sort of grouping which will enable us the better to deal with a large number of things. Were this all, the matter would not be very complex, for such an arrangement might be obtained in many different ways and without any very extensive knowledge. One might very well be content with dividing plants up into trees and shrubs and herbs and so on. Or animals might be divided into those which fly, those which live in the water, those which are useful to man, and so on. Such a system might enable one

actually to arrive at identifications and might serve for purely practical purposes. It is in fact about the sort of classification that the average non-biological individual employs. The lumberman, for example, in the classification of the different kinds of trees does little better than this.

Such a system is, of course, capable of much refinement without at all changing its fundamental character. With the increasing keenness and knowledge of classifiers it may even come to be a rather good expression of degrees of resemblance. Nevertheless, it must always remain from the scientific point of view a very superficial and inadequate system. A scientific system demands the weighing and sifting of the evidence in order to arrive at some actual connection, causal if possible, between the things that are placed together.

In biology this connection is furnished by the theory of evolution. With its acceptance our systems of classification ceased to be merely a pigeon-holing of objects into groups that looked more or less alike. They became an attempt actually to express the degree of relationship of one form to another, in other words something of an attempt to measure the distance which species have diverged in the course of their evolutionary development. They became an attempt to express the actual phylogeny of organisms.

This idea remains the controlling conception in modern taxonomy. Our system is expected to group organisms together into various categories of different rank, proceeding step after step until the branches of the tree have been carried back to their common root.

It is perfectly evident that our attempts to develop a system in accord with such a conception are greatly handicapped. In the first place, we too often have nothing but the end results of the processes that have gone on throughout the ages. True, the situation is not quite so bad as it may seem, for those processes have left their indelible mark upon even these end results, a mark that at times can clearly be deciphered. Furthermore there are fragments of the earlier records in the form of fossils.

Another difficulty in connection with a system of categories higher than the species is identical with one which we have discussed in connection with the species itself. It is that of gradation. If we had at hand a complete series of all the forms through which living organisms have passed in the ages of time we should—theoretically at least—have a complete series of steps, probably of such small degree that any division which we might make would be purely arbitrary. Actually we do not have anywhere such a series and so the problem is somewhat simplified; but even so it is still a serious obstacle.

It will appear immediately that inasmuch as the series of steps, and

consequently the degrees of relationships, through which existing species have passed have been almost infinite, it would require a correspondingly large number of categories to express those relationships with any degree of accuracy. On the other hand, practical considerations demand that the number of categories must be decidedly limited. So we come immediately to the root of many of our difficulties.

There is, on the one hand, the purely scientific and theoretical aim of so analyzing the data presented by the study of species as to express the degrees of relationship, to form a genuinely phylogenetic classification; and there is, on the other hand, the practical aim of facilitating the identification of species.

The conflict between these two aims may at times become quite acute. The demands for "convenience," for a classification that shall be simple and easy to remember, and—above all—for a terminology that shall be stable, become opposed to the demands of abstract theory which is concerned merely with the attempt to arrive at a theoretically ideal arrangement. This ideal system might demand an extensive series of categories and, as progress takes place, a shifting nomenclature, while the practical aims are well satisfied with simple genera, families, and orders, and with almost any name that may be readily available.

Which of these conflicting views may rule? From the point of view of the writer, neither may. The result must of necessity be something of a compromise. The taxonomist who is dealing with a group of practical importance should not, in his devotion to abstract principle, lose sight of the fact that his work, if it is to be used, must be usable. Nor, on the other hand, should the practical worker in his desire for simplicity and usableness forget that the theoretical taxonomist has legitimate aims of his own. To divide a genus artificially merely for the sake of "convenience" is the negation of scientific method. If there are actually five hundred species in any group so closely related that their relationships can best be expressed by keeping them in a single genus it were inexcusable from any scientific point of view to break up the genus merely to get smaller groups.

Again, the whole matter is complicated by certain purely psychological factors. Some types of mind are more impressed by one aspect of relationship than by another. There are those whose first impulse upon being shown two species would be to say, "How much alike they are," and there are those who would say, "How different they are." So the one type will strive to emphasize differences and the other resemblances. The one will call for continually more restricted categories and the other will insist upon broader limits.

The extent of our knowledge is also involved. The older authors who knew relatively little of the things with which they were dealing could with a clear conscience place together in a single order such things as insects and pill bugs. But increasing knowledge leads to a realization of the artificiality of such an arrangement and brings about restriction. The same factor is still at work. As long as no one knew anything much about the sucking lice they could be placed in three or four genera and regarded as a sub-order of the Hemiptera. But as soon as the knowledge of the group began to increase it became perfectly evident that such an arrangement, both as to number and composition of the genera and ordinal placing was not in accord with the facts. Even comparatively recent entomological textbooks group together into a single order, Aptera, a long series of forms which competent students now consider to be but remotely related. The probabilities are that these students are much more nearly correct in their views than are those who would hold to the single order. All too often the conservative objects to the "excessive splitting" of some familiar group simply because he is insufficiently informed concerning it.

In addition to these differences, there are also differences in practice as to the categories which shall be recognized. The Hymenopterist has long been accustomed to the use of the term "superfamily," but there are other groups of the insects in which the term is almost unknown. There are taxonomists who accept and make frequent use of the term "subgenus," but there are others who will have nothing of it. The student passing from one group to another and from the literature of one language to that of another will find a wide divergence of usage. He will find such terms as "series," "legion," "phalanx," variously employed.

As far as the terminology of the various categories is concerned the most generally accepted practice has more or less agreed upon the following steps, beginning with the class: class, subclass, order, suborder, superfamily, family, subfamily, tribe, genus, subgenus, species, subspecies. Occasionally superordinal and subtribal groups are recognized. Within the genus, subordinate to the subgenus there may be recognized "groups." This term has no nomenclatorial standing and it takes the form of, for example, "the group of species A."

It would appear that by standardizing upon this system, employing consistently super- and sub-groups of each primary category, it should be possible to arrive at a system that is sufficiently extended and flexible for even the theoretical systematist. It would contain, beginning with the class and ending with the subspecies and including the nomenclatorially unrecognized but very useful "group," seventeen categories, as follows: class, subclass, superorder, order, suborder, superfamily, family, sub-

family, supertribe, tribe, subtribe, supergenus, genus, subgenus, group, species, subspecies.[1] Some of these categories have been used but little, yet their employment would lead to the development of a logical system.

For the purposes of the non-specialist, however, it is by no means necessary to make use of any such extensive terminology. All that is necessary is to select a restricted number of terms, such as order, family, genus, species, which will adequately meet the ordinary needs.

Such a system can be applied to any group of organisms, making it a simple matter for the student to transfer from one to another. As an example of awkwardness we may cite the steps leading to the spiny lobsters in the Class Crustacea, sub-class Malacostraca, series Eumalacostraca, division Eucarida, order Decapoda, sub-order Reptantia, section Palinura, tribe Scyllaridea, as given in Calman's *Life of Crustacea.* The advantages of a uniform system are obvious.

As to the content of these various categories, no clear-cut statement can be made. Whether genera and families or any other category should be large or small is unfortunately simply a matter of individual judgment. The student, confronted with the problem, can only balance the data and attempt to arrive at a reasonable middle course, avoiding on the one hand the dangers of "splitting" and on the other those of "lumping."

It is evident that if we are trying to express degrees of relationship our purpose may be frustrated by going to either of the possible extremes. To make genera so large and all inclusive that even the use of subgeneric and "group" categories will still not indicate groupings is to obscure by undue conservatism. To make genera so small that there is no room for subgenera even is again to fail of our purpose. It is quite true that in the latter case we might conceivably shift the values of higher categories, making the tribe serve the purpose for which the genus ordinarily is employed; but there would seem to be no gain in such a procedure.

As an example of the effect of undue conservatism, or lack of knowledge, let us consider the case of the order Mallophaga. The writer has dealt with this group[2] and may quote from one of his papers:

> The genera are for the most part simply heterogeneous assemblages of superficially similar forms held together by a flimsy structure of artificialities and awaiting a drastic dividing and delimiting to be of any particular significance. All these conditions are well illustrated in the group of species included in Harrison's admirable catalogue of the Mallophaga under the family Meno-

[1] The use of the term "superspecies" has been avoided here because of possible undesirable implications.

[2] Ferris, G. F., "The Mallophagan Family Menoponidae. Part I," *Parasitology,* XVI (1), 56, (1924).

ponidae. The catalogue lists 453 species in this family—nearly one-fourth of all the species of Mallophaga—distributed among twelve genera. There are three genera which are not included in this catalogue and the inclusion of which would change the figures somewhat, but if we take the catalogue as a basis we find 414 of the 453 species comprised within three genera, *Menopon* with 192, *Colpocephalum* with 166, and *Myrsidea* with 56. The largest of the other nine genera contains but eleven species. There are no subgenera recognized under these large genera, nor are there any keys to the included species. Concealed within them there are unquestionably a considerable series of groups of species, the recognition of which will be necessary in order to bring out the facts of distribution and relationship.

Here it is only by judicious division that the data can best be expressed. The existing classification conceals the very things which it should reveal.

As an example of the other extreme, Dipterists would probably agree in taking the work upon the Muscoid flies. Here the genera and the families as well have been divided to the point where it seems that all expression of relationship has disappeared. And yet it may very well be that the conventional classification of these flies goes too far in the other direction.

The touchstone of it all is degree of relationship. Whatever system will best reveal the apparent phylogeny of the forms with which we deal, that is the system that should be employed. One thing is certain: no system which does not take into consideration all the evidence that can be obtained can be regarded as anything more than a temporary makeshift.

We may in this connection examine a certain curious phenomenon having to do with the sizes of genera.

It is a strange and at present unaccounted-for fact that if we plot on co-ordinate paper the number of genera contained in any group against the number of species contained in those genera, using one axis for genera and the other for species, we get, apparently regardless of the group with which we are dealing and the quality of the systematic work done upon that group, a curve of a certain characteristic form. This has been called the "hollow curve of distribution."

Attention was first called to this curve by Willis,[3] an English botanist, who utilized it as the basis for certain conclusions that need not concern us here. Chamberlin has considered it in a somewhat extended paper,[4] but the subject seems worthy of still further investigation. All that may be said here is intended merely as a suggestion of possible usefulness.

The characteristics of this curve can be seen by reference to the accompanying figure (Fig. 11). It will be noted that there are in general more

[3] Willis, J. C., *Age and Area*, Cambridge University Press, 1922.

[4] Chamberlin, J. C., "The Hollow Curve of Distribution," *American Naturalist*, LVIII, 350–374, (1924).

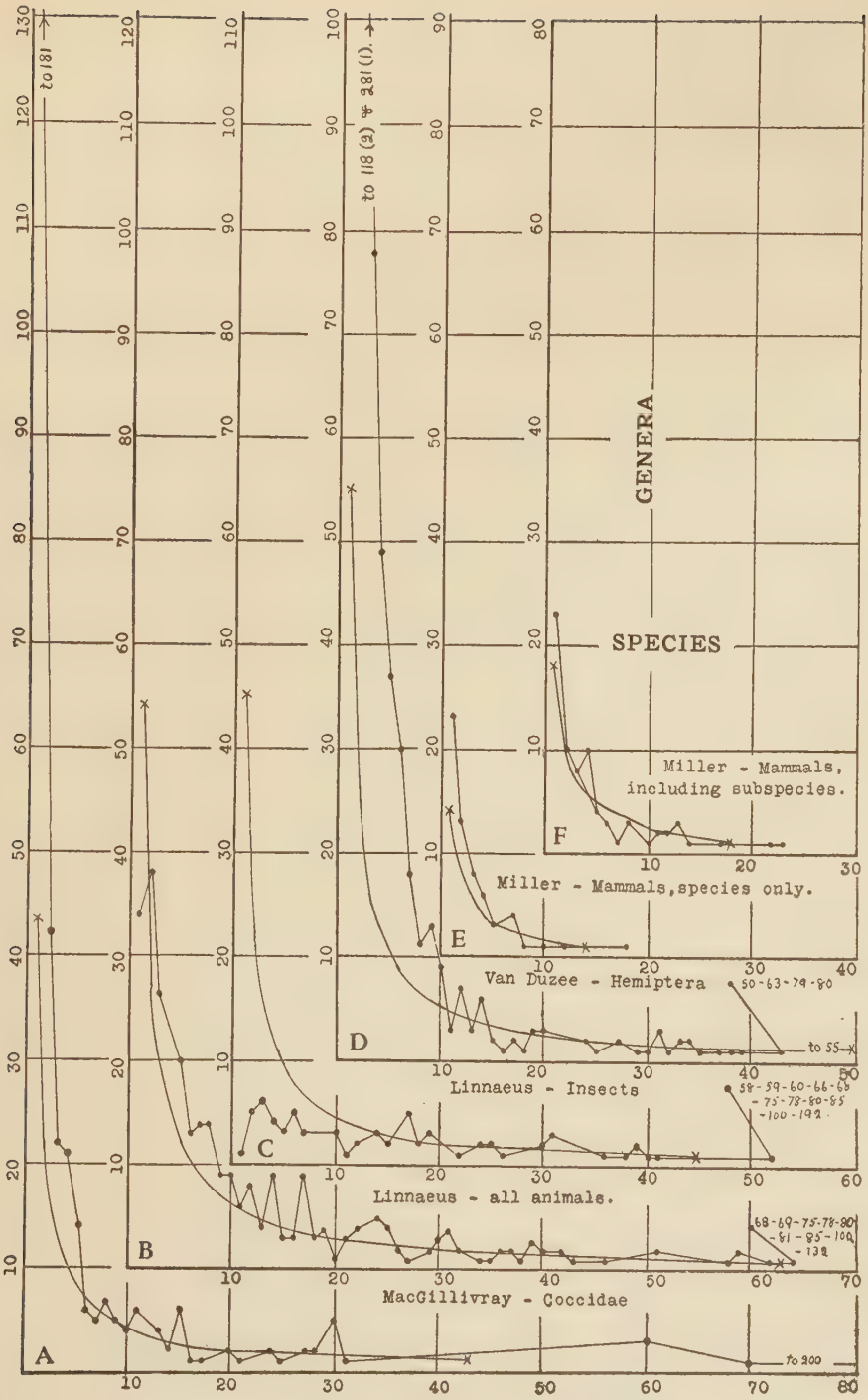

FIG. 11.—Graphs illustrating the "hollow curve of distribution."

genera of a single species than there are of two, more of two species than of three, and so on. This condition holds except in certain special cases, and even in these cases it applies in part. The result is that, examined superficially, these curves appear in all cases to be very much alike. It was realized by Chamberlin that this resemblance is largely superficial, and an attempt was made to obtain some standard with which they might be compared.

Concerning this Chamberlin says:

. . . . it was seen that some standard basis must be devised for the comparison of such curves. For this purpose, what I am calling the ideal curve was obtained in two independent ways. In the first place, the hyperbolic appearance of the curve suggested that the simple formula of $XY = K$ would satisfy the conditions. As a corollary to this it is obvious that the number of species in any given generic class should theoretically be equal to the number of *species* in any other generic class. Thus, if we have a group with 100 monotypic genera we should theoretically have 50 ditypic genera, 33 tritypic genera, and finally a single genus of 100 species. Consequently the ideal curve for any group under consideration may be obtained by simply totaling the number of species included and extracting the square root of the sum. (This is the method by which all the ideal curves in the accompanying figures were obtained.) This root, then, is equal to the K of our formula and determines the limits of our curve.

The second derivation of the ideal curve was deduced from purely theoretical conditions with the following ideas as guides:

(1) Other things being equal, phyletic differentiation and spatial distribution will vary directly with time.

(2) The ideal curve for any type of phenomena may be obtained by divesting it of all modifying factors and carrying the progression to its logical mathematical conclusion.

Thus, in order to obtain the simplest possible expression for the evolution of genera (or species), we may assume an original genus which gives rise by mutation to a new genus for each regular time unit or interval, which rate is effective, also, as applied to the mutant or daughter genera. This is a simple case of geometrical progression, mathematically expressible by the formula,

$$a + (a + 1)^1 + (a + 1)^2 + (a + 1)^3 + \ldots\ldots,$$

in this case a being equal to one. It is the type of increase displayed by a regularly dichotomously branching diagram or by the reproductive activities of a single-celled organism increasing by simple binary fission. In other words we have simply expanded this type of reproduction to account for the origin of genera and species.

Then, by assuming that species arise at the same rate and by the same method outlined above for the genera, and carrying our progression for convenience sake to the number 64 and plotting the sizes of the genera obtaining at this particular point, we find that the eight values we obtain coincide exactly

with points on the ideal form of the curve, as determined by using 64 as our constant or curve limit in our first method. . . . . This close correlation, then, seems to entirely justify the use of such a curve as a standard for our purpose.[5]

By the use of this curve, which we shall call the "comparison curve," it is possible to show that the apparent similarity of the various curves obtained from different groups is superficial even though striking. A mathematical analysis of the differences could undoubtedly be developed but is unnecessary for our immediate purpose.

The question, for our purposes, is whether these curves have any significance to the systematist. The writer is inclined to the view that they may have. We need not enter into the reasons for the appearance of this curve so regularly in spite of the varying conditions of our systematic work. Whether it is an expression of biological fact or whether it arises merely from our methods of work—and the writer considers that both are involved—is not extremely important, at least for the moment. The question is whether there is any correlation between the form of this curve and the character of our work, and conversely whether we may possibly utilize the comparison curve as to some extent a check upon our methods.

Let us consider merely the curves in the accompanying figure.

If we take the curves derived from Linnaeus' *Systema Naturae,* we find an interesting condition. In both these curves (B, C), the departure from the comparison curve is especially marked because of the deficiency of small genera, there being a far smaller number of monotypes than the comparison curve calls for, and the excessive number of large genera, which far surpasses the number called for by the comparison curve. The change in our point of view since the time of Linnaeus, leading to a tremendous increase in the proportion of small genera, is beautifully shown by the comparison of the Linnean curves with two other curves derived from recent work. One of these (Fig. 11A) is based upon MacGillivray's book, *The Coccidae,* and the other (Fig. 11D) is based upon Van Duzee's *Catalogue of the Hemiptera of America North of Mexico.* Note that in the curve of Linnaeus' insects there is but one monotypic genus, while the comparison curve calls for fifty-five, while in the curve for the Coccidae there are about four and one-half times as many monotypes as are called for by the comparison curve and in the curve of the Hemiptera there are a little over five times as many. Something of the same proportions will be found in almost any curve based upon recent work.

Regardless of the views of those who plead for a "return to Linnean

[5] Chamberlin, *op. cit.*

genera," the fact remains that Linnaeus really knew very little about the organisms with which he dealt. The deficiency of small genera, coupled with the excessive number of large genera, in the curve of his insects is in all probability a result of this lack of knowledge. Note that curve B, which is based upon all his animals including insects, approaches much more closely the more modern trend, although even here there is a deficiency of small genera and an excess of large. The difference between the curve of the insects and that of all the animals is due to the nature of the curves of the birds, mammals, reptiles, and fishes, which more nearly approach the comparison curve. It is very probable that the Linnean classification of the birds and mammals was actually a better expression of the biological facts than was that for the insects.

Concerning the classification of the Hemiptera in general the writer is not qualified to speak, but concerning that of the Coccidae he feels himself on safe ground. Almost all students of the Coccidae will agree that in the curve drawn from MacGillivray's book there is an entirely undue proportion of small genera and the number of monotypes is very excessive. Certainly, also, the large genera of this curve are unnatural groups. The writer does not doubt that a really sound generic grouping of the Coccidae would approach much more closely the comparison curve, at both ends at least, than does that here shown.

In Fig. 11E is shown a curve derived from Miller's *Catalogue of the Land Mammals of Western Europe*. There is little room for question as to the quality of this work. Its author is one of the leading mammalogists of the world and the volume probably represents some of the soundest systematic work that is to be found. The number of species dealt with is somewhat small, but note the close correspondence of the curve with its comparison curve. There are a little less than two times as many monotypes as the comparison curve indicates, and the large genera are by no means far outside of the limits of the latter. In Fig. 11F is shown the curve derived from the same data, but counting subspecies as species. Note the even closer correspondence, with an increase in the excess of large genera.

There is in all this much food for thought. There is, for one thing, the very clear picture of the change in our concepts since the time of Linnaeus. Bear in mind that nearly every one of the larger genera, at least, recognized by Linnaeus is now anything from a family to even an order. Note the excessive number of small genera in the curve of the Coccidae. Here again, the departure from the comparison curve is probably an expression of error. Note the correspondence of the two curves derived from the work of Miller, which is undoubtedly good work.

It may very well be that while we are by no means justified in using this comparison curve as a standard to which our work should be forced to conform we may be justified in using it as a means of checking abnormal departures from a reasonable system. It certainly will not tell us in any individual case whether two genera should be united or another divided, but it may indicate to us that our work as a whole needs to be reviewed and examined critically. There is certainly in it an indication—which the present writer has been led from entirely different reasons to accept as valid—that large genera are likely to be due to lack of knowledge and an excess of small genera to a lack of judgment.

All that has been said concerning these curves and their possible application to systematic work is intended merely by way of suggestion. The curves and their underlying causes and significance need to be studied much further. But the writer does believe that they present possibilities of usefulness. The study of the work of systematists by such methods should lead to some interesting results.

We have not yet, however, arrived at anything which will tell us exactly what a genus should be. Nor, at the present time, can anyone say what it should be; and because of this circumstance systematics are in a most peculiar position. We are trying to measure something—that something being the degree of phylogenetic differentiation—but we cannot agree upon the standard of measurement!

Concerning all this the writer wishes to quote from an unpublished manuscript of J. C. Chamberlin. The genus, he says,

> is of course a concept or idea which has no material counterpart anywhere in objective nature. It is a category or class upon which we set purely arbitrary limits. In this respect it is of course identical with all other basic scientific units and standards.
>
> The genus, family, etc., while units in a true scientific sense, are fundamentally different from most other such categories in that they are standards of *phylogenetic and genetic similarity or affiliation,* instead of standards of *dimension, mass, or energy.* They cannot in any sense be made derivatives of these latter fundamental physical standards. There exists a basic and necessary difference between the two classes. But although we fail to define these systematic groups in terms of time, length, and mass, which involve the true fundamental units of physical and chemical science, there is no *a priori* reason why they can not be defined upon the basis of other and just as fundamental concepts. By this I mean the definition of our systematic units in terms of ideas as basic in their significance as time, mass, and extension.
>
> Species are of course dependent upon the individuals which comprise them. Individuals arise through what we term reproduction and without this continued organic life becomes an impossibility.
>
> In a similar way we must grant species reproduction or speciogenesis. Unless

this is so, of course, organic evolution is a fiction. Species must arise and they must arise from some pre-existing stocks or "proto-species." Species, too, have their parentage.

Genealogy deals with the relationship of individuals with respect to their birth or genesis. *Phylogeny is the genealogy of species.*

Speciogenesis would seem in all probability to be a dichotomy, a dividing of a single species into two. The basis for this perhaps startling assumption seems, in the light of all data thus far available, a perfectly reasonable and safe one, depending as it does upon the facts of ordinary reproduction—which intrinsically *involve the segregation of a discrete entity from an originally single substance*—and the empirical observation that in nearly all thoroughly developed studies species are found to have one definitely closest relative and not two or more, as any hypothesis not including the idea of dichotomy must necessarily imply. In other words, speciogenesis is probably in the great majority of cases entirely analogous to binary fission in the one-celled animals.

Against this view of the mode of origin of species objections will of course be urged. It may, for example, be maintained that species actually arise as it were by explosion, that is by the shattering of pre-existing species into a group of forms, rather than by its division into two. However that may be—and there is no one theory of speciogenesis to which all biologists are ready to subscribe—we may, for the purposes of discussion, adopt this view of their dichotomous origin as one way of approach to our problem of genera. It would take much space to develop the detailed argument supporting it, and that we may pass over with the assurance that there is such argument. This argument is entirely consistent with the discussion given in a previous chapter concerning the morphological basis of systematic entomology.

Chamberlin continues:

It is sufficient for our purposes to call attention to the fact that there is considerable evidence to show that morphology is directly correlated with speciogenesis and evolution in general. It is upon this assumption that all natural systems of classification not based upon actual observations of time and origin must openly or tacitly depend.

We have practically no dates or time records of the birth of species as we have of individuals in ordinary pedigrees. The few exceptions are those recorded by paleontology. Consequently true time relations are unknown in most biological classifications. Hence, in devising our "natural" systems of classification, we are dependent solely upon morphology and anatomy and rarely physiology in establishing our time or age factor.

It is evident that the age or date of birth of a species can no more be depended upon as a measure of relationship than would be the case with individuals. A grandson may easily be, in point of time alone, actually older than a son, for example. The essential difference is not in the date of birth but in the

sequence of generations. There is no doubt that the same thing is true in phylogeny.

On the other hand, when we find a morphological character extending through a great number of groups or species we believe them to be related and we believe that the character has had a common origin. We naturally and logically assume then that this character is *genetically* older than all the thus related species. It is consequently on this principle that all natural classifications operate. On this basis we reconstruct our phylogeny, keeping in mind constantly that the time axis is purely relative and indeterminate, except through occasionally preserved fossil evidence.

This principle may be expressed as follows. *A species—or any other aggregate—is phylogenetically younger than any character which it shares with other species or other aggregates and hence this character is definitive of an aggregative category of higher degree.*

It is evident that these aggregative characters are of all degrees and extent. One may be common to but two species, another to three, another to four, while another may bind together ten thousand or another a million species.

If our theory of the dichotomous origin of species be accepted as valid, even for purposes of discussion, we note at once a very significant correlation between it and the above conclusions. We are permitted to formulate certain important generalizations.

(1). There are theoretically as many aggregative systematic units as there have been dichotomies or fissions of the original stock. The aggregation next to that of the species will be two species, the next higher four species, the next eight, the next sixteen, and so on. Hence, granting for the moment that we have absolutely every existing species at hand for study, a character grouping three species together would represent a theoretical four, one combining five would represent at least eight, and so on. The strict dichotomy might early be upset because some species may be less prolific than others, but *potentially* the whole system would represent a dichotomous system. One branch, for example, might contain its full quota of eight species, while its alternate branch might contain but one. On this basis the single species is genetically the equivalent of the other eight. Some of the published protozoan pedigrees illustrate this point most admirably.

To be strictly logical, *each dichotomy is a group or category of species,* whether we *name* the category or not. If this be granted as valid, a perfectly logical and biologically sound definition of the genus can be given.

(2). A genus is a purely arbitrary conception, in the same sense as the meter is a purely arbitrary standard of extension. But this is no real defect. What is alone necessary to give such a standard validity is *agreement.* If every man set up and used his own standard for the meter, independently of every other man, we would have exactly the condition that now appears in systematic work. Agreement on a single standard is essential to order.

(3). From this we may conclude that, if the above principles be accepted, within their limits we may define a standard for the genus, which if generally accepted, would be as definite as is the meter. A maximum convenient number

of species, consistent with a practical arrangement of categories and with the aim of revealing relationships could be accepted as constituting the theoretical genus. This would need to be some number of a dichotomous progression, one hundred and twenty-eight, let us say.

On this basis a positive definition of the genus can be formulated as follows: A genus is that systematic biological category which contains *potentially* one hundred and twenty-eight species, all of which are included within a maximum limit of twelve subdichotomies; or it is a number of species which may be assumed to represent not more than this number of related dichotomies.

The determination of generic limits on this basis would be simpler than may at first sight appear. The two most closely related species in a group could be used as a standard of the ultimate dichotomy and by working backward on this basis the other dichotomies naturally follow.[6]

All of this is without doubt a purely theoretical discussion and against the ideas contained within it there may be many objections. Nevertheless it is a fresh point of view and it indicates one line of thought that may point a way out of a portion of the present difficulties. The writer knows of no other attempt to place the whole subject upon such a clear-cut and definite basis.

As far as the higher categories are concerned much the same situation exists. These too, at present, are simply concepts, changing from time to time and from worker to worker. But with these the situation is not so difficult, for these higher categories are much more academic than is the genus. They are relatively few in number, and in the main they are more definitely separable from each other. There are, for example, but a little more than a hundred families of beetles, while there are thousands upon thousands of species and hundreds upon hundreds of genera. It is evident that it is with genera that the most serious thought is demanded, for it is with genera that the majority of systematists stop. There are but few who have ever named a family, and not one in a thousand, as McAtee has remarked, will ever have occasion even to consider the naming of an order, but nearly every systematist has named from one to many genera.

It is then with the genus that we are primarily concerned in any attempt at classification. If the genera are properly composed and well defined the grouping of them into supergenera and subtribes and tribes and so on throughout the entire series of categories will be comparatively simple.

What is there to offer then as a definite formula that a systematist, confronted by the problem of the constitution of genera may use as a guide? It must be confessed that there is but little. The difficulties are inherent in the task that he has undertaken, that of measuring with the aid

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[6] Chamberlin, unpublished MS.

of a series of abstract concepts a thing of which but fragments are available for measurement. It is in large part a task in which all those factors—mostly imponderable—that together make up what we call judgment are vitally concerned. Such a factor as the extent and character of the systematist's general biological training and his comprehension of general biological problems enters in. The systematist who has a general biological training and the systematist who knows only that certain kinds of insects make attractive cabinet specimens to which names are attached are not likely to arrive at any common solution. Such a factor as controlled imagination, involving the ability to see beyond the bare facts that are immediately available is necessary to raise it all above the dead level of drudgery and routine. And undoubtedly such a factor as intuition is at times present. So much is all of this true that at times it would seem that those factors which are definite and capable of concrete expression are largely subordinated.

Nevertheless, there are some things which have been partially foreshadowed in the foregoing discussion and which will aid the student in forming his opinions, although they may not in any particular case tell him definitely whether any particular group is or is not a genus.

It has already been indicated—but it may here be repeated with greater emphasis—that *the essence of classification is grouping.* The genus is essentially a *group* of closely related species. It is clear then that the tendency should be rather to hold species together—to conserve groups—than to separate them, unless that separation be the separation of *groups.* In other words, the monotypic genus, *in general,* is undesirable. This is not to say that monotypic genera should never be named, for there unquestionably are numerous forms so isolated from their nearest *known* relatives that to insist on keeping them attached to another group merely to avoid naming a monotypic genus would merely obscure the fact of their divergence. But it is the writer's conviction that the proportion of genuinely monotypic genera in the world is comparatively small, that is, that there really is but a relatively small proportion of species so isolated that they actually have no relative close enough to be called congeneric.

It is probably true, then, that even if a genus be based upon but a single species, it is only a matter of time until some other species will be found to go along with it. Especially does this seem probable in view of the fact that we may actually not have recognized at the present time more than ten per cent of the total number of insect species in the world. In fact nearly every genus probably has begun with a very small number of species. The monotypic genus is then a *potential* group, and it is merely as such that it is in the majority of cases justifiable. Nevertheless, the proportion of

monotypic genera seems to be increasing, and if that increase and the tendency toward continued splitting and the continued degrading of the genus be carried on indefinitely it is evident that there will not be enough potential groups possible to reduce the proportions to their proper limits.

It is evident also that our conception of groups must shift as our knowledge increases. We find, let us say, a dozen species that are evidently related to each other but which differ quite markedly from each other. What do those dozen species represent? Is each of them actually a nucleus for what will eventually prove to be a genuine group, a genus? Or are all of the dozen merely representatives of a single group and all to be placed in a single genus?

It seems to the writer that a practical treatment of this problem is this: Out of the dozen species there may possibly be one or two so markedly different from the others that they may in all probability represent what should be called a distinct genus, a potential group. The others may very well be held together in a single genus, but if it is possible to do so subgenera should be named and species groups indicated. By doing this the facts of difference have been indicated, and if increased knowledge later shows that these species groups and subgenera are worthy of elevation to higher rank that step can very easily be taken.

In other words, all that any attempt at a classification can do is to say that *at any given state of our knowledge* the relative degree of relationship of a series of forms appears to be thus and so. That relative degree will shift as knowledge increases. Categories will be added at the lower end of the series, the base of the pyramid will be pushed up, the species group, or the subgenus or the genus of today becoming the subgenus, or the genus, or the tribe of tomorrow.

The indiscriminate naming of monotypic genera does not lend itself to the maintenance of any such relation of the various levels. Its tendency is to reduce all to a single level, and the only way by which a graded system can be restored is to recombine these monotypic genera again into groups. Such a procedure, however, means the complete squeezing out of the very useful and very expressive subgenus and species group, and thus it limits the number of available categories, when what is needed to express relationship is really an expansion of the series.

On the other hand, when a genus reaches any very large size it is almost certain to contain within itself groups of various degrees of value. We may, by using the subgenus and the species group, still express relative degrees of relationship, but if additional groupings appear we are at a loss and must either invent more categories or cut down the genus. When this stage has been reached it would appear to be time to begin division.

These, then, would seem to be quite definite and understandable criteria, requiring only that knowledge of the organisms which any competent systematist should have and the possession of some degree of good judgment.

Another undesirable course is that of naming groups on the basis of characters which will distinguish but one sex. As McAtee has remarked:

> The use of sexual characters for defining genera also is to be deplored. It is as much as can be endured to use these characters of which we do not actually know the significance, for the differentiation of what we take the liberty of calling species. Often we must put aside as an unknown mass specimens of the sex which does not show our 'characters,' or if both sexes have strong 'characters' likely enough we give actual mates distinct 'specific' names. Such usage, we repeat, is bad enough in the case of 'species,' and should be entirely taboo for the much more important category of genus. If we cannot place all sex forms of an insect in their proper genus on structural characters, then we have too many genera.[7]

This, he adds, does not mean "necessarily the same characters, nor does this remark apply to groups where in general only one sex is available for study." With all that he says concerning this the present writer is in complete agreement.

Another undesirable course is that of naming significant groups solely on the basis of purely adaptive characters. Such characters may possibly be of value in arranging artificial keys simply for identification, but that is another matter. An excellent example of what is meant by this is the following: There is in the scale insects (Coccidae or Coccidoidea) a curious genus, *Margarodes,* all the members of which are subterranean forms. The adults, both female and male as far as the latter is known in the various species, have the forelegs enormously enlarged and adapted for digging. Largely on this basis this genus has been made a subfamily, equivalent in rank to a number of other strongly marked and clearly defined groups. As a matter of fact the genus really belongs—on the basis of what unquestionably are significant structures—to one of these other groups. The enlarged forelegs are purely an adaptive character and have no phylogenetic significance outside of perhaps defining in part the genus.

As to what is or what is not an adaptive character in any particular case, that is a question in which the student's general biological knowledge is likely to be called upon. It may at times be a very dubious matter, for there are modern biologists who will question seriously the rôle of adapta-

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[7] McAtee, W. L., "Insect Taxonomy: Preserving a Sense of Proportion," *Proceedings, Entomological Society of Washington,* XXVIII, 3: 68–70, (1926).

tion, and undoubtedly the subject has at times been unduly stressed. Nevertheless, there do appear to be clear cases, such as that cited above.

As to what are or what are not subgeneric or generic or supergeneric characters, no rule of any sort can be laid down, or if any rule is laid down one must be ready at any moment to see it fail in its application. In one family certain characters will obviously define groups that may be called generic. In another those same characters, while present, will apparently be useless, either because of their fixity on the one hand or their evident plasticity on the other. Nothing could be more misleading than the attempt to be thoroughly consistent in such matters. Such consistency is impossible, for organisms themselves refuse to be consistent. What, for example, would seem more important and fundamental than the occurrence of wings in the insects? And yet there are species in which this fundamental character, distinctive of an entire class, may either be present or absent! *It is especially because of this very inconsistency of the organisms themselves that the naming of a new group for every notable departure from the typical form of an existing group is likely to obscure the actual relationship.*

Examples to illustrate this last point are numerous. The author may perhaps be pardoned for drawing another illustration from the scale insects, as that is the group with which he is most familiar. In what are commonly called the Diaspidine scales there are found in the region of the vulva certain groups of pores from which there issues a powdery wax that coats the eggs. It would appear that the presence of these groups of pores is an important character and probably it is, so much so that its absence has been utilized as the basis for generic separations. Yet we have some curious results. There exist certain species, for example *Aspidiotus rapax* Comstock and *A. lataniae* (Signoret), which are almost identical in every respect except that in one these groups of pores are lacking. These species, however, have been placed in separate genera. The writer considers this procedure to be entirely misleading. It is of the same nature as would be the unthinkable course of ruling out of the Insecta all those forms that do not have wings! The determination of groups, then, is a matter not of fixing upon some certain structure which is to be regarded as definitive. It is a matter of balancing a series of structures and weighing their relative importance *in the particular case in hand.* And all of this requires some knowledge of the material with which one is working.

And so, although we may not lay down any formula by which the limits of any group may be determined, we may at least develop some ideas which will aid the student in forming his opinions. We may at least

hope that those who are interested in these problems will think a bit about them. To quote again from McAtee, "To contribute to a desirable type of phylogentic tree, what is mainly needed is exercise of a sense of proportion."

Through it all it were well to remember that there are two things involved. One of them is the knowledge of the organisms. The other is an attempt to arrange that knowledge into a system. It is evident that the basic thing is knowledge and that without it no system which may be devised is likely to be either valid or enduring.

CHAPTER XI

NOMENCLATURE

As a part of the practical work of the systematist the utilization of a system of nomenclature is necessary and this system, in order to be generally usable, must be governed by a system of rules by which systematists everywhere and always should abide.

We have referred in an earlier chapter to the fact that the mere giving of names to organisms has in times past assumed an undue importance in systematic work and that even yet it occasionally does so. Nevertheless, it is not the intention of the writer to deprecate the real importance of this activity. We cannot dispense with names. They are an essential part of the equipment with which we must work. Some system by means of which we can refer to the things with which we deal must be employed. It has, therefore, been merely the intention of the writer to emphasize the idea that this system of naming is merely a tool, not an end in itself, and that consequently it needs to be restrained within its legitimate bounds and to be considered in its due relation to those ends which it properly serves.

The writer wishes also to emphasize the view that any one who is properly to be called a systematist must consider it a part of his equipment for his profession to know something of the commonly accepted rules and the procedure that governs the application of names.

The invention of the existing system of nomenclature provided systematists with one of the most important tools that they possess, and yet this whole matter of the naming of organisms remains one of the most unsatisfactory things with which the systematist has to deal.

Consider the way in which our present body of systematic literature has come into being. It began at a time when actual knowledge of organisms was extremely slight and when the instruments by means of which a greater knowledge could be obtained did not exist. It has been added to by men of all degrees of knowledge and ability, of all nations in every corner of the earth. The results of their studies have been published in a host of books and journals and in the languages of all civilized peoples. Is it any wonder that under such conditions difficulties should have arisen? The surprising thing really is that there has been as much co-ordination as there has been. Undoubtedly the powerful influence of the nomenclatorial system devised by Linnaeus can be given much, if not nearly all, of the credit for this.

Ideally every species should have a name by which it may be known at all times and by all people. Ideally that name should remain fixed

or as nearly fixed as is practically possible. But under the conditions through which our systematic work has been compelled to develop, such an ideal has been unattainable. Could each worker have had available all the literature of all times and of all languages, it could not yet have been entirely attained. The isolation of workers, the obscurity of much of the literature, the differences in language have created further obstacles quite impossible to overcome.

In addition to these difficulties, with our growing knowledge it becomes evident that the earlier workers had in many cases entirely too broad conceptions of the nature of species. Our standards have changed to such an extent that much of the earlier work is almost incomprehensible. In addition to these changes in point of view there is the circumstance, to which we have already alluded at some length, that much systematic work has been done so badly that the application of names cannot be properly determined. Then, too, there have been differences of opinion as to what constitutes a species. There have been countless errors. There have been changes in our equipment.

On top of all this the list of names has been steadily growing. Where Linnaeus knew but seventy-two genera of insects, we now have hundreds within the limits of a single family. Where he knew not ten thousand species of all kinds of animals we now know twenty thousand species of beetles alone in North America north of Mexico. The flood of names has been overwhelming. It has poured down upon systematic workers until they have literally been deluged beneath it. It would have been difficult enough to keep the nomenclature in order even if it had been entrusted to a single central office, operating under modern ideals of efficiency and with a trained personnel. As it is, the result has been in many cases an almost hopeless degree of confusion.

Throughout the course of all this, however, some attempt to keep at least a certain degree of order has been made. Nomenclature, as an abstract thing apart, has for much of the time since Linnaeus been operated under rules of some sort. Obviously, however, such rules could be nothing more than those which were adopted by mutual consent or which obtained some weight because of the influence of some master worker. There never has been, and is not now, any organization or any group of individuals who can say with the authority of law what must be done. The best that at any time has been available is a more or less effective public opinion as expressed through organizations of systematists.

It were beside the point here to go into any great detail in considering the development of nomenclature, but a brief résumé of the subject will perhaps aid the student in developing an appreciation of it.

Scientific nomenclature as we now know it began with—one might quite properly say was invented by—one man, Linnaeus, less than two hundred years ago. Let us go back to the times that immediately preceded him.

The long sterile period of the Middle Ages was over. Reliance upon Aristotle as the sole source of information in regard to the world of nature had given way to the study of nature itself. Even a certain amount of experimental work had been introduced and for a hundred years there had been no lack of writers who had set out to describe the phenomena of nature. Some of them were men of the very highest capacity, men who fairly deserve to rank as geniuses. Even today one cannot but marvel at how much many of them were able to accomplish with the inadequate equipment that they possessed. On the whole, in spite of the deficiencies of mechanical equipment and the other difficulties which they faced, they had accumulated a respectable amount of information.

It should be remembered that at this time Latin was the language of all educated people. It probably came as near to being a universal language as the world has ever possessed. So it naturally followed that the writings of learned men were usually in Latin. In the course of these writings names were given to organisms. These names were descriptive phrases, or sentences, or even paragraphs. So when one wished to speak of an organism for which there was no common name there was no convenient and simple means of reference.

The discovery of the way out of this difficulty was due to Linnaeus. To him occurred the idea that these Latin names might be reduced to two words, one to indicate the general kind, the genus, the other for the particular kind, the species. In a series of publications, both in Botany and Zoölogy, he established his methods and set out to list and name all the known forms in accord with this system.

It should be remembered that Linnaeus himself was not personally familiar with even the comparatively small number of forms that he listed. His method was that of gathering together a list of all the organisms that had been described by earlier authors and adding to this all that he himself knew. Of the titles under which the results of his labors were published we are especially concerned with but one. This is the *Systema Naturae Regnum Animale,* which contained the lists of names and short descriptions of all the animals known, together with a classification of them into larger groups. This book passed through several editions, each succeeding edition containing additional names and in some cases changing names that had appeared in earlier editions. Zoölogists have, by unanimous consent, agreed to accept the tenth edition,

published in 1758, as the starting-point of our nomenclature. It was in this edition that the binomial system was first definitely established, at least as far as Zoölogy is concerned.

Back of this date, then, scientific nomenclature is considered not to exist. Names are accepted as proposed in this volume, dating from the time of its publication, and they are credited to Linnaeus, even though he himself may simply have recorded forms described by earlier authors. Furthermore, no scientific names appearing since that date and not conforming to the principles of the binomial system are accepted.

There have of course been changes in our concepts since the time of Linnaeus. The idea of Evolution had never occurred to Linnaeus, and consequently his classification attempted merely to express likeness, which, in the absence of a guiding principle, led to some curious results. But it was the peculiar virtue of the binomial system that it lent itself equally well to any changes which the conception of evolution brought about, and the revolution of biological thought caused no corresponding revolution in biological systematics. In addition to this change in the point of view there have been others in the details of method. The simple binomial system has been expanded to admit of the subspecies or variety, so that it is now binary and trinomial, and in some extreme cases this multiplication of names has been carried even farther. Examples can be found, in botanical literature at least, of as many as five names. Our conceptions of the composition and the terminology of groups higher than the genus have also changed. Where Linnaeus employed only class, order, genus, and species, we now have the lengthy series of groups that have been discussed in an earlier chapter. From the extremely broad and inclusive generic groupings of Linnaeus we have come steadily to smaller and more restricted groups. The genus of Linnaeus may be now as much as a superfamily or even an order. A reference to the graphs in Fig. 11 will show how marked this tendency has been.

Through it all, however, the underlying principles of the Linnean system have remained unchanged, nor is there any immediate prospect of any radical departures from them. At various times it has been suggested that numerical systems, similar perhaps to the Dewey system of library cataloguing, might be developed, but nothing has come of this and it is difficult to see how anything can come of it.

There was of course nothing to compel the acceptance of the Linnean system. Its general excellence and usability alone have been the reasons for its survival. Simple as it is, however, in its fundamentals there immediately arise a great many questions as to the proper procedure to be followed in its application. Linnaeus himself recognized the desirability

of some general rules of procedure and at various places in his published works, notably in the *Philosophia Botanica* of 1751, promulgated rules.

Nevertheless, there was for many years nothing in the way of a generally accepted code of procedure, although individuals at times attempted to formulate codes. Fabricius's *Philosophia Entomologica* of 1778 is said to have contained the first set of rules published for entomological nomenclature. Rudolphi, a parasitologist, proposed a set of rules in 1801, but he is said not to have followed his own recommendations consistently.

All these codes, however, are purely personal affairs and it was apparently not until 1842 that any attempt was made to devise what might be called an "official" code. This code was sponsored by the British Association for the Advancement of Science, which in 1842 appointed a committee to consider the subject. The report of this committee was published in the report of the meeting of the British Association for June of that year and the code has since been known as the "Stricklandian Code," from the name of one of the members of the committee. It is said that the code was first officially adopted by an American organization, the American Society of Geologists, in 1845, not being officially adopted by the British Association until the next year.

This code was republished in 1863, by the British Association, and utilized by the same organization as the basis for an amended code of 1865. This code is regarded as the basic source from which our modern codes have been derived. "It had," says one writer, "great influence for good, and did much to bring zoölogical nomenclature from a loose and almost chaotic state to a fair degree of stability and orderly consistency. Its principal defects are those which could not then have been perceived and avoided . . . ." The code was followed for a number of years by both American and British zoölogists.

At this point there was a decided step forward. With the promulgation of the Stricklandian Code and its acceptance by certain scientific organizations the matter of nomenclatorial procedure ceased to be a purely personal affair. It became to some extent at least a matter of concerted action.

In 1876, W. H. Dall, a conchologist, was appointed by Section B of the American Association for the Advancement of Science as a committee of one "to obtain an expression from the working naturalists of America, in regard to the nature of a set of rules for facilitating the decision of questions relating to nomenclature." His report was published in 1877, but was never formally adopted by the Association.

In 1881 a code was adopted by the Zoölogical Society of France and in

this same year there appeared the first code of an international nature. The International Congress of Zoölogy adopted what is known as the Douvillé Code, to govern the names of fossils.

In 1885 the American Ornithologists' Union prepared a code which was based upon the Stricklandian Code, with the changes necessary to adapt it to later conditions. One of these changes was the provision for trinomials. This code has been extremely influential, and it is worth while to note that the ornithologists are probably the most closely organized of any of the biological groups.

So far the codes, with the exception of the Douvillé Code, have been at the most national in their scope, although the influence of some of them was international as well. We come now to the period of the development of truly international codes.

To the initiative of Raphael Blanchard, a French zoölogist, we owe the beginnings of the present International Code, which is, in theory at least, accepted by all zoölogists. Blanchard proposed a code which was adopted at the First International Zoölogical Congress at Paris in 1889 and was re-adopted at the Second Congress in Moscow in 1892.

The Germans, however, objected to this code and in 1894 proposed a code of their own. The situation in 1895, to quote from Stiles, was this. "English systematists were following the Stricklandian Code; French systematists were following the International Code; German systematists were following the German Code; American systematists were divided between the Stricklandian, the 'A.O.U.', the Dall, and the International codes; systematists in special groups were in some cases following special or even personal codes; and systematists of Italy, Russia, and some other countries were following either the International or some other code." It may very well be suspected that there were in addition those who were following no code at all.

At the Third International Zoölogical Congress at Leyden in 1895 an international committee of five members was appointed to study all these codes and to consider their similarities and differences. At the Fourth Congress at Cambridge in 1898 this committee was increased to fifteen members and at the Fifth Congress in Berlin, in 1901, the code proposed by this committee was adopted. The committee has been continued since that time as a permanent body.

The powers of the International Commission, as summed up by Stiles,[1] are as follows:

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[1] Stiles, C. W., "The International Code of Nomenclature as Applied to Medicine," *United States Treasury Department, Hygienic Laboratory Bulletin No. 24,* 1905.

Under the ruling of the International Congress, no proposition for change in the code is permitted to come before the congress, unless it is presented to the Permanent Commission at least one year before the meeting of the congress. Any person in the world has the right to make any nomenclatural proposition, and this is first considered by the executive committee; this committee studies the proposition and then submits it to the commission; the commission considers it and makes recommendations to the congress, which has the final decision in the matter. By this method of procedure it is intended to provide for any additions to the code which may become necessary, but to protect the code from the introduction of any revolutionary and ill-advised changes.

Thus the commission is a deliberative and advisory body, but it has no legislative powers; legislation rests with the congress, and membership in this is open to all persons interested in zoölogy, regardless of country or special field of work.

It would, accordingly seem that the general scheme of legislative action is well-arranged and that provision is made both to insure conservatism and to provide for progress.

Neither the commission nor the congress has any power to force zoölogists and others to adopt the International Rules. Every person is still in a position to follow any code he desires or to prepare one of his own, but, considering the responsibilities involved, there is undoubtedly a growing tendency to adopt the International Code. Further, it is considered ethical to ignore names used contrary to this and to other standard codes.

There has been some objection to the International Code. At the time of this writing a British National Committee on Entomological Nomenclature is engaged in the attempt to formulate a revised and simplified code. To any such action there is the very pertinent objection that entomology after all is but a part of zoölogy and the utilization of specialized codes within this restricted field is likely to result merely in the same confusion from which nomenclature was so long in escaping.

It is not the intention here to enter into any discussion of the principles underlying the International Code, but for the convenience of entomological students the code is here reprinted, together with such of the decisions of the International Commission as have any bearing upon entomological problems. The code and the abstracts of decisions as here given are taken from the most recent publication in which they are presented,[2] with the addition of the amendments to Article 25 which were adopted at the Zoölogical Congress at Prague, Hungary, in September, 1927. The decisions which are here omitted have to do with matters of no entomological interest, such as names in other groups which are to be included in the official list of Zoölogical names.

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[2] *Proceedings Biological Society of Washington,* XXXIX, 75–104, (1926).

INTERNATIONAL RULES OF ZOÖLOGICAL NOMENCLATURE

GENERAL CONSIDERATIONS

ARTICLE 1.—Zoölogical nomenclature is independent of botanical nomenclature in the sense that the name of an animal is not to be rejected simply because it is identical with the name of a plant. If, however, an organism is transferred from the vegetable to the animal kingdom its botanical names are to be accepted in zoölogical nomenclature with their original botanical status; and if an organism is transferred from the animal to the vegetable kingdom its names retain their zoölogical status.

Recommendation.—It is well to avoid introducing into zoölogy as generic names such names as are in use in botany.

ARTICLE 2.—The scientific designation of animals is uninominal for subgenera and all higher groups, binominal for species, and trinominal for subspecies.

See Opinions Nos. 19, 20, 24, 35, 43, 46, 50, 54.

ARTICLE 3.—The scientific names of animals must be words which are either Latin or Latinized, or considered and treated as such in case they are not of classic origin.

FAMILY AND SUBFAMILY NAMES

ARTICLE 4.—The name of a family is formed by adding the ending *idae,* the name of a subfamily by adding *inae,* to the stem of the name of its type genus.

ARTICLE 5.—The name of a family or subfamily is to be changed when the name of its type genus is changed.

GENERIC AND SUBGENERIC NAMES

ARTICLE 6.—Generic and subgeneric names are subject to the same rules and recommendations, and from a nomenclatural standpoint they are co-ordinate, that is, they are of the same value.

See Opinion No. 72.

ARTICLE 7.—A generic name becomes a subgeneric name, when the genus so named becomes a subgenus, and *vice versa.*

ARTICLE 8.—A generic name must consist of a single word, simple or compound, written with a capital initial letter, and employed as a substantive in the nominative singular. Examples: *Canis, Perca, Ceratodus, Hymenolepis.*

Recommendation.—Certain biological groups which have been proposed distinctly as collective groups, not as systematic units, may be treated for convenience as if they were genera, but they require no type species. Examples: *Agamodistomum, Amphistomulum, Agamofilaria, Agamomermis, Sparganum.*

Recommendations.—The following words may be taken as generic names:

a) Greek substantives, for which the rules of Latin transcription [transliteration (see Appendix F)] should be followed. Examples: *Ancylus, Amphibola, Aplysia, Pompholyx, Physa, Cylichna.*

b) Compound Greek words, in which the attributive should precede the principle word. Examples: *Stenogyra, Pleurobranchus, Tylodina, Cyclostomum, Sarcocystis, Pelodytes, Hydrophilus, Rhizobius.*

This does not, however, exclude words formed on the model of *Hippopotamus,* namely, words in which the attributive follows the principal word. Examples: *Philydrus, Biorhiza.*

c) Latin substantives. Examples: *Ancilla, Auricula, Dolium, Harpa, Oliva.* Adjectives (*Prasina*) and past participles (*Productus*) are not recommended.

d) Compound Latin words. Examples: *Stiliger, Dolabrifer, Semifusus.*

e) Greek or Latin derivatives expressing diminution, comparison, resemblance, or possession. Examples: *Dolium, Doliolum; Strongylus, Eustrongylus; Limax, Limacella; Limacia, Limacina, Limacites, Limacula; Lingula, Lingulella, Lingulepis, Lingulina, Lingulops, Lingulopsis; Neomenia, Proneomenia; Buteo, Archibuteo; Gordius, Paragordius, Polygordius.*

f) Mythological or heroic names. Examples: *Osiris, Venus, Brisinga, Velleda, Crimora.* If not Latin these should be given a Latin termination. (*Aegirus, Göndulia*).

g) Proper names used by the ancients. Examples: *Cleopatra, Belisarius, Melania.*

h) Modern patronymics, to which is added an ending to denote dedication:

α. Names terminating with a consonant take the ending *ius, ia,* or *ium.* Examples: *Selysius, Lamarckia, Köllikeria, Mülleria, Stålia, Krøyeria, Ibañezia.*

β. Names terminating with the vowels *e, i, o, u,* or *y* take the ending *us, a,* or *um.* Examples: *Blainvillea, Wyvillea, Cavolinia, Fatioa, Bernaya, Quoya, Schulzea.*

γ. Names terminating with *a* take the ending *ia.* Example: *Danaia.*

δ. In generic names formed from patronymics, the particles are omitted if not coalesced with the name, but the articles are retained. Examples: *Blainvillea, Benedenia, Chiajea, Lacepedea, Dumerilia.*

ε. With patronymics consisting of two words, only one of these is used in the formation of a generic name. Examples: *Selysius, Targionia, Edwardsia, Duthiersia.*

ζ. The use of proper names in the formation of compound generic names is objectionable. Examples: *Eugrimmia, Buchiceras, Heromorpha, Möbiusispongia.*

i) Names of ships which should be treated the same as mythological names (*Vega*) or as modern patronymics. Examples: *Blakea, Hirondellea, Challengeria.*

j) Barbarous names, that is words of nonclassic origin. Examples: *Vanikoro, Chilosa.* Such words may receive a Latin termination. Examples: *Yetus, Fossarus.*

k) Words formed by an arbitrary combination of letters. Examples: *Neda, Clanculus, Salifa, Torix.*

l) Names formed by anagram. Examples: *Dacelo, Verlusia, Linospa.*

ARTICLE 9.—If a genus is divided into subgenera, the name of the typical subgenus must be the same as the name of the genus (see Art. 25).

ARTICLE 10.—When it is desired to cite the name of a subgenus, this name is to be placed in parentheses between the generic and the specific names. Examples *Vanessa (Pyrameis) cardui.*

SPECIFIC AND SUBSPECIFIC NAMES

ARTICLE 11.—Specific and subspecific names are subject to the same rules and recommendations, and from a nomenclatural standpoint they are co-ordinate, that is, they are of the same value.

ARTICLE 12.—A specific name becomes a subspecific name when the species so named becomes a subspecies, and *vice versa.*

ARTICLE 13.—While specific substantive names derived from names of persons may be written with a capital initial letter, all other specific names are to be written with a small initial letter. Examples: *Rhizostoma Cuvieri* or *Rh. cuvieri, Francolinus Lucani* or *F. lucani, Hypoderma Diana* or *H. diana, Laophonte Mohammed* or *L. mohammed, Œstrus ovis, Corvus corax.*

ARTICLE 14.—Specific names are:

a) Adjectives, which must agree grammatically with the generic name. Example: *Felis marmorata.*

b) Substantives in the nominative in apposition with the generic name. Example: *Felis leo.*

c) *Substantives in the genitive.* Examples: *rosae, sturionis, antillarum, galliae, sancti-pauli, sanctae-helenae.*

If the name is given as a dedication to one or several persons, the genitive is formed in accordance with the rules of Latin declination in case the name was employed and declined in Latin. Examples: *plinii, aristotelis, victoris, antonii, elisabethae, petri* (given name).

If the name is a modern patronymic, the genitive is always formed by adding, to the exact and complete name, an *i* if the person is a man, or an *ae* if the person is a woman, even if the name has a Latin form; it is placed in the plural if the dedication involves several persons of the same name. Examples: *cuvieri, möbiusi, nuñezi, merianae, sarasinorum, bosi* (not *bovis*), *salmoni* (not *salmonis*).

Recommendation. The best specific name is a Latin adjective, short, euphonic, and of easy pronunciation. Latinized Greek words or barbarous words may, however, be used. Examples: *gymnocephalus, echinococcus, ziczac, aguti, hoactli, urubitinga.*

It is well to avoid the introduction of the names *typicus* and *typus* as new names for species or subspecies, since these names are always liable to result in later confusion.

See Opinions Nos. 8, 50, 64.

ARTICLE 15.—The use of compound proper names indicating dedication, or of compound words indicating a comparison with a simple object does not form an exception to Art. 2. In these cases the two words composing the specific name are written as one word with or without the hyphen. Examples: *Sanctae-Catharinae* or *sanctaecatharinae, jan-mayeni* or *janmayeni, cornu-pastoris* or *cornupastoris, cor-anguinum* or *coranguinum, cedo-nulli* or *cedonulli.*

Expressions like *rudis planusque* are not admissible as specific names.

See Opinion No. 50.

ARTICLE 16.—Geographic names are to be given as substantives in the genitive, or are to be placed in an adjectival form. Examples: *sancti-pauli, sanctae-helenae, edwardiensis, diemenensis, magellanicus, burdigalensis, vindobonensis.*

Recommendation.—Geographic names used by the Romans or by Latin writers of the Middle Ages are to be adopted in preference to more recent forms. Words like *bordeausiacus* and *viennensis* are poor, but are not to be rejected on this account.

ARTICLE 17.—If it is desired to cite the subspecific name, such name is written immediately following the specific name, without the interposition of any mark of punctuation. Example: *Rana esculenta marmorata* Hallowell, but not *Rana esculenta* (*marmorata*) or *Rana marmorata* Hallowell.

ARTICLE 18.—The notation of hybrids may be given in several ways; in all cases the name of the male parent precedes that of the female parent, with or without the sexual signs:

a) The names of the two parents are united by the sign of multiplication (×). Example *Capra hircus* ♂ × *Ovis aries* ♀ and *Capra hircus* × *Ovis aries* are equally good formulae.

b) Hybrids may also be cited in form of a fraction, the male parent forming the numerator and the female parent the denominator. Example: $\frac{Capra\ hircus}{Ovis\ aries}$. This second method is in so far preferable that it permits the citation of the person who first published the hybrid form as such. Example: $\frac{Bernicla\ canadensis}{Anser\ cygnoides}$ Rabé.

c) The fractional form is also preferable in case one of the parents is itself a hybrid.

Example: $\frac{Tetrao\ tetrix \times Tetrao\ urogallus}{Gallus\ gallus}$.

In the latter case, however, the parentheses may be used. Example: (*Tetrao tetrix* × *Tetrao urogallus*) × *Gallus gallus*.

d) When the parents of a hybrid are not known as such [parents] the hybrid takes provisionally a specific name, the same as if it were a true species, namely, as if it were not a hybrid; but the generic name is preceded by the sign of multiplication. Example: × *Coregonus dolosus* Fatio.

FORMATION, DERIVATION, AND ORTHOGRAPHY OF ZOÖLOGICAL NAMES

ARTICLE 19.—The original orthography of a name is to be preserved unless an error of transcription, a *lapsus calami,* or a typographical error is evident.

See Opinions Nos. 8, 26, 27, 29, 34, 36, 41, 60, 61, 63, 70.

Recommendation.—For scientific names it is advisable to use some other type than that used for the text. Example: "*Rana esculenta* [italics] Linné, 1758, lives in Europe."

ARTICLE 20.—In forming names derived from languages in which the Latin alphabet is used, the exact original spelling, including diacritic marks, is to be retained. Examples: *Seleysius, Lamarckia, Köllikeria, Mülleria, Stâlia, Krøyeria, Ibañezia, möbiusi, medići, čžjžeki, spitzbergensis, islandicus, paraguayensis, patagonicus, barbadensis, färöensis.*

Recommendations.—The prefixes *sub* and *pseudo* should be used only with adjectives and substantives, *sub* with Latin words, *pseudo* with Greek words, and they should not be used in combination with proper names. Examples: *subviridis, subchelatus, Pseudacanthus, Pseudophis, Pseudomys.* Words like *sub-wilsoni* and *pseudo-grateloupana* are not recommended.

The terminations *oides* and *ides* should be used in combination only with Greek or Latin substantives; they should not be used in combination with proper names.

Geographic and patronymic names from countries which have no recognized orthography or which do not use the Latin alphabet should be transcribed into Latin according to the rules adopted by the Geographic Society of Paris. (See Appendix G).

In proposing new names based upon personal names, which are written sometimes with ä, ö, or ü, at other times with ae, oe, and ue, it is recommended that authors adopt ae, oe, and ue. Example: muelleri, in preference to mülleri.

AUTHOR'S NAME

ARTICLE 21.—The author of a scientific name is that person who first publishes the name in connection with an indication, a definition, or a description, unless it is clear from the contents of the publication that some other person is responsible for said name and its indication, definition, or description.

ARTICLE 22.—If it is desired to cite the author's name, this should follow the scientific name without interposition of any mark of punctuation; if other citations are desirable (date, *sp. n., emend., sensu stricto,* etc.) these follow the author's name, but are separated from it by a comma or by parenthesis. Examples: *Primates* Linné, 1758, or *Primates* Linné (1758).

Recommendation.—When the name of the author of a scientific name is abbreviated, the writer will do well to conform to the list of abbreviations published by the Zoölogical Museum of Berlin.[3]

ARTICLE 23.—When a species is transferred to another than the original genus or the specific name is combined with any other generic name than that with which it was originally published, the name of the author of the specific name is retained in the notation but placed in parentheses. Examples: *Taenia lata* Linné, 1758, and *Dibothriocephalus latus* (Linné, 1758); *Fasciola hepatica* Linné, 1758, and *Distoma hepaticum* (Linné, 1758).

If it is desired to cite the author of the new combination, his name follows the parentheses. Example: *Limnatis nilotica* (Savigny, 1820) Moquin-Tandon, 1826.

ARTICLE 24.—When a species is divided, the restricted species to which the original specific name of the primitive species is attributed may receive a notation indicating both the name of the original author and the name of the reviser. Example: *Taenia solium* Linné, partim, Goeze.

THE LAW OF PRIORITY

ARTICLE 25.—The valid name of a genus or species can be only that name under which it was first designated on the condition:

a) That (prior to January 1, 1931) this name was published and accompanied by an indication, or a definition, or a description; and

b) That the author has applied the principles of binary nomenclature.

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[3] *Liste der Autoren zoologischer Art-und Gattungsnamen zusammengestelt von den Zoologen des Museum für Naturkunde in Berlin,* Berlin, 2. vermehrte Auflage, 8°, 1896.

c) But no generic name nor specific name, published after December 31, 1930, shall have any status of availability (hence also of validity) under the Rules, unless and until it is published either

1) with a summary of characters (seu diagnosis; seu definition; seu condensed description) which differentiate or distinguish the genus or the species from other genera or species;

2) or with a definite bibliographic reference to such summary of characters (seu diagnosis; seu definition; seu condensed description). And further

3) in the case of a generic name, with the definite unambiguous designation of the type species (seu genotype; seu autogenotype; seu orthotype).

Recommendations.—(a) It is requested that an author who publishes a name as new shall definitely state that it is new, that this be stated in only one (*i.e.*, in the first) publication, and that the date of publication be not added to the name in its first publication.

(b) It is requested that an author who *quotes* a generic name, or a specific name, or a subspecific name, shall add at least once the author and year of publication of the quoted name or a full bibliographic reference.

See Opinions Nos. 1, 2, 4, 5, 9, 10, 12, 13, 15–17, 19–21, 24, 28, 37–40, 46, 49–54, 56–59, 65–67, 73–78, 84, 85, 87, 88, 90.

APPLICATION OF THE LAW OF PRIORITY

ARTICLE 26.—The tenth edition of Linné's *Systema Naturae,* 1758, is the work which inaugurated the consistent general application of the binary nomenclature in zoölogy. The date 1758, therefore, is accepted as the starting-point of zoölogical nomenclature and of the Law of Priority.

See Opinions Nos. 3, 12, 13, 15, 16, 51, 52.

ARTICLE 27.—The Law of Priority obtains and consequently the oldest available name is retained:

a) When any part of the animal is named before the animal itself;

b) When any stage in the life history is named before the animal itself;

c) When the two sexes of an animal have been considered as distinct species or even as belonging to distinct genera;

d) When an animal represents a regular succession of dissimilar generations which have been considered as belonging to different species or even to different genera.

See Opinions Nos. 44, 88.

ARTICLE 28.—A genus formed by the union of two or more genera or subgenera takes the oldest valid generic or subgeneric name of its components. If the names are of the same date, that selected by the first reviser shall stand.

The same rule obtains when two or more species or subspecies are united to form a single species or subspecies.

Recommendation.—In absence of any previous revision, the establishment of precedence by the following method is recommended:

a) A generic name accompanied by specification of a type has precedence over a name without such specification. If all or none of the genera have types specified, that generic name takes precedence the diagnosis of which is most pertinent.

b) A specific name accompanied by both description and figure stands in preference to one accompanied only by a diagnosis or only by a figure.

c) Other things being equal, that name is to be preferred which stands first in the publication (page precedence).

See Opinion No. 40.

ARTICLE 29.—If a genus is divided into two or more restricted genera, its valid name must be retained for one of the restricted genera. If a type was originally established for said genus, the generic name is retained for the restricted genus containing said type.

Recommendation.—To facilitate reference it is recommended that when an older species is taken as type of a new genus, its name should be actually combined with the new generic name in addition to citing it with the old generic name. Example: *Gilbertella* Eigenmann, 1903, Smithsonian Misc. Coll., v. 45, p. 147, type *Gilbertella alata* (Steindachner) = *Anacyrtus alatus* Steindachner.

See Opinion No. 10.

ARTICLE 30.—The designation of type species of genera shall be governed by the following rules (*a*—*g*), applied in the following order of precedence:

See Opinions Nos. 11, 14, 18, 23, 31–33, 42, 43, 45, 62, 68, 69, 71, 79, 81, 86.

I. Cases in which the generic type is accepted *solely* upon the basis of the original publication:

a) When in the original publication of a genus, one of the species is definitely designated as type, this species shall be accepted as type, regardless of any other considerations. (Type by original designation.) (*See Opinion* No. 7.)

b) If in the original publication of a genus, *typicus* or *typus* is used as a *new* specific name for one of the species, such use shall be construed as "type by original designation."

c) A genus proposed with a single original species takes that species as its type. (Monotypical genera.) (*See Opinions* Nos. 6, 9, 22, 30, 42, 47.)

d) If a genus, without originally designated (see *a*) or indicated (see *b*) type, contains among its original species one possessing the generic name as its specific or subspecific name, either as valid name or synonym, that species or subspecies becomes *ipso facto* type of the genus. (Type by absolute tautonymy.) (*See Opinions* Nos. 16, 33, 35.)

II. Cases in which the generic type is accepted not solely upon basis of the original publication:

e) The following species are excluded from consideration in determining the types of genera. (*See Opinions* Nos. 14, 32, 35, 56.)

α. Species which were not included under the generic name at the time of its original publication.

β. Species which were *species inquirendae* from the standpoint of the author of the generic name at the time of its publication.

γ. Species which the author of the genus doubtfully referred to it.

f) In case a generic name without originally designated type is proposed as a substitute for another generic name, with or without type, the type of

either, when established, becomes *ipso facto* type of the other. (*See Opinions* Nos. 9, 46.)

g) If an author, in publishing a genus with more than one valid species, fails to designate (see *a*) or to indicate (see *b, d*) its type, any subsequent author may select the type, and such designation is not subject to change. (Type by subsequent designation.) (*See Opinions* Nos. 6, 9, 10, 32, 56.)

The meaning of the expression "select the type" is to be rigidly construed. Mention of a species as an illustration or example of a genus does not constitute a selection of a type.

III. *Recommendations.*—In selecting types by subsequent designation authors will do well to govern themselves by the following recommendations:

h) In case of Linnean genera, select as type the most common or the medicinal species. (Linnean rule, 1751.)

i) If a genus, without designated type, contains among its original species one possessing as a specific or subspecific name, either as valid name or synonym, a name which is virtually the same as the generic name, or of the same origin or same meaning, preference should be shown to that species in designating the type, unless such preference is strongly contraindicated by other factors. (Type by virtual tautonymy.) Examples *Bos taurus, Equus caballus, Ovis aries, Scomber scombrus, Sphaerostoma globiporum;* contraindicated in *Dipetalonema* (compare species *Filaria dipetala,* of which only one sex was described, based upon one specimen and not studied in detail.)

j) If the genus contains both exotic and nonexotic species from the standpoint of the original author, the type should be selected from the nonexotic species.

k) If some of the original species have later been classified in other genera, preference should be shown to the species still remaining in the original genus. (Type by elimination.)

l) Species based upon sexually mature specimens should take precedence over species based upon larval or immature forms.

m) Show preference to species bearing the name *communis, vulgaris, medicinalis,* or *officinalis.*

n) Show preference to the best-described, best-figured, best-known, or most easily obtainable species, or to one of which a type specimen can be obtained.

o) Show preference to a species which belongs to a group containing as large a number of the species as possible. (De Candolle's Rule.)

p) In parasitic genera, select, if possible, a species which occurs in man or some food animal, or in some very common and widespread host species.

q) All other things being equal, show preference to a species which the author of the genus actually studied at or before the time he proposed the genus.

r) In case of writers who habitually placed a certain leading or typical species first as "chef de file," the others being described by comparative reference to this, this fact should be considered in the choice of the type species.

s) In case of those authors who have adopted the "first species rule" in fixing generic types, the first species named by them should be taken as the types of their genera.

t) All other things being equal, page precedence should obtain in selecting a type.

ARTICLE 31.—The division of a species into two or more restricted species is subject to the same rules as the division of a genus. But a specific name

which undoubtedly rests upon an error of identification cannot be retained for the misdetermined species even if the species in question are afterwards placed in different genera. Example: *Taenia pectinata* Goeze, 1782 = *Cittotaenia pectinata* (Goeze), but the species erroneously determined by Zeder, 1800, as "*Taenia pectinata* Goeze" = *Andrya rhopalocephala* (Riehm); the latter species does not take the name *Andrya pectinata* (Zeder). (*See Opinion* No. 13.)

REJECTION OF NAMES

ARTICLE 32.—A generic or a specific name, once published, cannot be rejected, even by its author, because of inappropriateness. Examples: Names like *Polyodon, Apus, albus,* etc., when once published, are not to be rejected because of a claim that they indicate characters contradictory to those possessed by the animals in question.

ARTICLE 33.—A name is not to be rejected because of tautonymy, that is, because the specific or the specific and subspecific names are identical with the generic name. Examples: *Trutta trutta, Apus apus apus.*

ARTICLE 34.—A generic name[4] is to be rejected as a homonym when it has previously been used for some other genus of animals. Example: *Trichina* Owen, 1835, nematode, is rejected as homonym of *Trichina* Meigen, 1830, insect.

See Opinions Nos. 12, 29, 83.

CODE OF ETHICS

Without presuming to be the arbiter of points of general ethics, the Commission is persuaded that there is one phase of this subject upon which it is competent to speak, and in reference to this point it suggests to the Congress the adoption of the following resolution:

Whereas—experience has shown that authors, not infrequently, inadvertently publish as new designations of genera or species, names that are preoccupied, *and*

Whereas—experience has also shown that some other authors, discovering the homonymy, have published new names for the later homonymns in question, be it *therefore*

RESOLVED—That when it is noticed by any zoölogist that the generic or specific name published by any living author as new is in reality a homonym, and therefore unavailable under Articles 34 and 36 of the Rules on Nomenclature, the proper action, from a standpoint of professional etiquette, is for said person to notify said author of the facts of the case, and to give said author ample opportunity to propose a substitute name.

ARTICLE 35.—A specific name is to be rejected as a homonym when it has previously been used for some other species or subspecies of the same genus. Example: *Taenia ovilla* Rivolta, 1878, (n. sp.), is rejected as a homonym of *T. ovilla* Gmelin, 1790.

[4] A homonym is one and the same name for two or more different things. Synonyms are different names for one and the same thing.

When in consequence of the union of two genera, two different animals having the same specific or subspecific name are brought into one genus, the more recent specific or subspecific name is to be rejected as a homonym.

Specific names of the same origin and meaning shall be considered homonyms if they are distinguished from each other only by the following differences:

a) The use of *ae, oe,* and *e,* as *caeruleus, coeruleus, cereuleus; ei, i,* and *y,* as *chiropus, cheiropus; c* and *k,* as *microdon, mikrodon.*

b) The aspiration or non-aspiration of a consonant, as *oxyryncus, oxyrhynchus.*

c) The presence or absence of a *c* before *t,* as *autumnalis, auctumnalis.*

d) By a single or double consonant; *litoralis, littoralis.*

e) By the endings *ensis* and *iensis* to a geographical name, as *timorensis, timoriensis.*

Article 36.—Rejected homonyms can never be used again. Rejected synonyms can be used again in case of the restoration of erroneously suppressed groups. Example: *Taenia giardi* Moniez, 1879, was suppressed as a synonym of *Taenia ovilla* Rivolta, 1878; later it was discovered that *Taenia ovilla* was preoccupied (*Taenia ovilla* Gmelin, 1790). *Taenia ovilla,* 1878, is suppressed as a homonym, and can never be used again; it was stillborn and can not be brought to life, even when the species is placed in another genus (*Thysanosoma*). *Taenia giardi,* 1879, which was suppressed as a synonym, becomes valid upon the suppression of the homonym *Taenia ovilla* Rivolta.

Recommendations.—It is well to avoid the introduction of new generic names which differ from generic names already in use only in termination or in a slight variation in spelling which might lead to confusion. But when once introduced, such names are not to be rejected on this account. Examples: *Picus, Pica; Polyodus, Polyodón, Polyodonta, Polyodontas, Polyodontus; Macrodon, Microdon.*

The same recommendation applies to new specific names in any given genus. Examples: *necator, necatrix; furcigera, furcifera; rhopalocephala, rhopaliocephala.*

If from the radical of a geographic name two or more adjectives are derived, it is not advisable to use more than one of them as specific name in the same genus, but if once introduced they are not to be rejected on this account. Examples: *hispanus, hispanicus; molluccensis, moluccanus; sinensis, sinicus, chinensis; ceylonicus, zeylanicus.*

The same recommendation applies also to other words derived from the same radical and differing from each other only in termination or by simple change in spelling.

SUSPENSION OF RULES IN CERTAIN CASES

RESOLVED.—That plenary power is herewith conferred upon the International Commission on Zoölogical Nomenclature, acting for this Congress, to suspend the Rules as applied to any given case, where in its judgment the strict application of the Rules will clearly result in greater confusion than uniformity, *provided,* however, that not less than one year's notice shall be given in any two or more of the following publications, namely, *Bulletin de la Société zoolo-*

gique de France, Monitore Zoologico, Nature, Science (N.Y.), and *Zoologischer Anzeiger,* that the question of a possible suspension of the Rules as applied to such a case is under consideration, thereby making it possible for zoölogists, particularly specialists in the group in question, to present arguments for or against the suspension under consideration; and *provided,* also, that the vote in Commission is unanimously in favor of suspension; and *provided,* further, that if the vote in Commission is a two-thirds majority of the full Commission, but not a unanimous vote in favor of suspension, the Commission is hereby instructed to report the facts to the next succeeding International Congress; *and*

RESOLVED.—That in the event that a case reaches the Congress, as hereinbefore described, with a two-thirds majority of the Commission in favor of suspension, but without unanimous report, it shall be the duty of the President of the Section on Nomenclature to select a special board of 3 members, consisting of one member of the Commission who voted on each side of the question and one ex-member of the Commission who has not expressed any public opinion on the case, and this special board shall review the evidence presented to it, and its report, either majority or unanimous, shall be final and without appeal, so far as the Congress is concerned; and

RESOLVED.—That the foregoing authority refers in the first instance and especially to cases of the names of larval stages and the transference of names from one genus or species to another; *and*

RESOLVED.—That the Congress fully approves the plan that has been inaugurated by the Commission of conferring with special committees from the special group involved in any given case, and that it authorizes and instructs the Commission to continue and extend this policy.

See Opinions Nos. 76, 80, 82, 89, 90.

APPENDIX

A.—It is very desirable that the proposition of every new systematic group should be accompanied by a diagnosis, both individual and differential, of said group in English, French, German, Italian, or Latin. This diagnosis should state in what museum the type specimen has been deposited and should give the museum [catalogue] number of said specimen.

It is recommended that in published descriptions of a new species or new subspecies, only one specimen be designated and labeled as *type,* the other specimens examined by the author at the same time being *paratypes.*

B.—In publications issued in any other language than English, French, German, Italian, or Latin, it is very desirable that the explanation of figures be translated into one of these tongues.

C.—The metric system of weights and measures and the centigrade thermometer of Celsius are adopted as standard. The *micron* (0.001 mm.) represented by the Greek letter μ, is adopted as the unit of measure in microscopic work.

D.—The indication of enlargement or of reduction, which is very desirable for the comprehension of an illustration, should be expressed in figures rather than by mentioning the system of lenses used.

E.—The indication of enlargement or reduction of an object is usually linear.

The sign of multiplication is used for enlargement and the fraction for reduction. Examples: 50 indicates that the object is enlarged 50 times. $\frac{1}{50}$ indicates that it is reduced to $\frac{1}{50}$th.

If it is desired to specify that the enlargement is linear, surface or mass, this may be done as follows: 50^1 indicates linear enlargement; 50^2 indicates surface enlargement; 50^3 indicates mass enlargement.

F.—*Transliteration of Greek Words.*—The following table indicates the manner in which Greek words should be transliterated:

ε = e	(ὑάλεος)	Hyal*e*a, not Hyal*ae*a
η = e	(πειρήνη)	Pir*e*na, not Pir*i*na
final η = a	(πειρήνη)	Pirena, not Pirene
ϑ = th	(τηθύς)	Te*th*ys, not Te*t*ys
ι = i	(βαλίος)	Bal*i*a, not Bal*e*a
ϰ = c	(ἱπποκρήνη)	Hippo*c*rena, not Hippo*ch*renes
ξ = x	(ξένος)	*X*enus, *X*enophora
ϱ = r	(πτερόν)	Pte*r*um
υ = y	(ὑβός)	H*y*bolithus, not H*i*bolites
αι = ae	(λιμναῖος)	Limn*ae*a, not Limn*e*a
αυ = au	(γλαυκός)	Gl*au*cus
ει = i	(χεῖλος)	Ch*i*lostomum, not Ch*ei*lostoma
ευ = eu	(εὖρος)	*Eu*rus
ῳ, οι = oe	(οἰκέω)	Di*oe*ca, Dendr*oe*ca, not Di*oi*ca, Dendr*oi*ca
final ον = um	(ἐφίππιον)	Ephippi*um*, not Ephippi*on*
final ος = us	(ὀμφαλός)	Euomphal*us*, not Euomphal*os*
ου = u	(λουτήριον)	L*u*terium, not L*o*torium
γγ = ng	(ἀγγαρεία)	*An*garia
γχ = nch	(ἀγχιστομον)	An*ch*istomum, not An*g*istoma
γϰ = nc	(ἄνκιστρον)	*An*cistrodon, not A*gk*istrodon
῾ϱ = rh	(῾ρέα)	*Rh*ea
ἑ = he	(ἑρμαῖα)	*He*rmaea, not *E*rmaea

G.—*Transcription of Geographic and Proper Names.*—The geographic names of nations which employ the Latin characters are to be written with the orthography of the country in which they originate.

The following paragraphs apply only to the geographic names of countries which have no true alphabet or which use letters that are different from the Latin alphabet.

Names of places, however, which have been established by long usage preserve their usual orthography. Examples: *Algiers, Moscow.*

1. The vowels *a, e, i,* and *o* are pronounced as in French, Italian, Spanish, or German. The letter *e* is never mute.

2. The French sound *u* is represented by *ü* with dieresis, as in German.

3. The French sound *ou* is represented by *u*, as in Italian, Spanish, German, etc.

4. The French sound *eu* is represented by *oe*, pronounced as in the French word *oeil.*

5. The long sound of a vowel is indicated by a circumflex accent; the interrupted sound is indicated by an apostrophe.

6. The consonants *b, d, f, j, k, l, m, n, p, q, r, t, v,* and *z* are pronounced as in French.

7. The letters *g* and *s* always have the hard sound, as in the French words *gamelle* and *sirop.*

8. The sound represented in French by *ch* is designated by *sh.* Examples: *Shérif, Kashgar.*

9. *Kh* represents the harsh guttural; *gh* represents the soft guttural of the Arabs.

10. *Th* represents the sound which terminates the English word *path.* (θ in Greek). *Dh* represents the sound which commences the English word *those* (ζ in Greek).

11. Aside from such employment (9, 10) of the letter *h* modifying the letter which precedes it, *h* is always aspirated, the apostrophe is therefore never used before a word commencing with *h.*

12. The semivowel represented by *y* is pronounced as in *yole.*

13. The semivowel *w* is pronounced as in the English word *William.*

14. The double sounds *dj, tch, ts,* etc., are indicated by letters representing the sounds which compose them. Example: *Matshim.*

15. The *ñ* is pronounced *gn,* as in seigneur.

16. The letters *x, c,* and *q* are not used, since they are duplicates of other letters representing the same sounds; but *q* may serve to indicate the Arabic *qaf* and the soft aspirate may be used to represent the Arabic *ain.*

An attempt should be made to indicate as exactly as possible, by means of the letters given above, the local pronunciation without trying to give a complete representation of all the sounds which are heard.

SUMMARIES OF OPINIONS RENDERED

1. *The Meaning of the Word "Indication" in Art. 25a.*—The word *"indication"* in Art. 25a is to be construed as follows:

A. With regard to *specific* names, an *"indication"* is (1) a bibliographic reference, or (2) a published figure (illustration), or (3) a definite citation of an earlier name for which a new name is proposed.

B. With regard to *generic* names (1) a bibliographic reference, or (2) a definite citation of an earlier name for which a new name is proposed, or (3) the citation or designation of a type species.

In no case is the word *"indication"* to be construed as including museum labels, museum specimens, or vernacular names.

2. *The Nature of a Systematic Name.*—The Commission is unanimously of the opinion that a *name,* in the sense of the Code, refers to the designation by which the actual objects are known. In other words, we name the objects themselves, not our conception of said objects. Names based upon hypothetical forms have, therefore, no status in nomenclature and are not in any way entitled to consideration under the Law of Priority.

Examples: *Pithecanthropus* Haeckel, 1866, being the name of a hypothetical genus, has no status under the Code, and does not therefore invalidate *Pithecanthropus* Dubois, 1894; *Gigantopora minuta* Loos, 1907, n.g., n. sp., has no status under the Code, since it is admittedly the name of a fantastic unit, not based upon any actual objects.

3. *The Status of Publications Dated 1758.*—The tenth edition of Linné's *Systema Naturae* was issued very early in the year 1758. For practical reasons, this date may be assumed to be January 1st, 1758, and any other zoölogical publications bearing the date 1758 may be construed as having appeared subsequent to January 1st. In so far as the date is concerned, all such publications may, therefore, be construed as entitled to consideration under the Law of Priority.

4. *Status of Certain Names Published as Manuscript Names.*—Manuscript names acquire standing in nomenclature when printed in connection with the provisions of

Art. 25, and the question as to their validity is not influenced by the fact whether such names are accepted or rejected by the author responsible for their publication.

5. *Status of Certain Pre-Linnean Names Reprinted Subsequent to 1757.*—A pre-Linnean name, ineligible because of its publication prior to 1758, does not become eligible simply by being cited or reprinted with its original diagnosis after 1757. To become eligible under the Code, such names must be reinforced by adoption or acceptance by the author publishing the reprint. Examples: The citation, subsequent to 1757, of a bibliographic reference to a paper published prior to 1758 does not establish technical names which may appear in said reference; synonymic citation of pre-Linnean names, as in the tenth edition of Linné's *Systema Naturae,* does not establish such names under the Code.

6. *In Case of a Genus A Linnaeus, 1758, with Two Species, Ab and Ac.*—When a later author divides the genus A, species Ab and Ac, leaving genus A only species Ab, and Genus C, monotypic, with species Cc, the second author is to be construed as having fixed the type of the genus A. (See Article 30).

7. *On the Interpretation of the Expression "n.g., n. sp." under* Article 30 (a).—The expression "n.g., n. sp.," used in publication of a new genus for which no other species is otherwise designated as genotype, is to be accepted as designation under Article 30 (a).

8. *On the Retention of ii or i in Specific Patronymic Names under Article 14 (c) and Article 19.*—Specific patronymics originally published as ending in *ii* (as *schrankii, ebbesbornii*) are, according to Article 19, to be retained in their original form, despite the provision of Article 14 (c), that they should have been formed with only one *i.*

9. *The Use of the Name of a Composite Genus for a Component Part Requiring a Name.*—The decision as to whether the name of a composite genus, when made up wholly of older gĕnera, is tenable for a component part requiring a name, depends upon a variety of circumstances. There are circumstances under which such a name may be used, others under which it may not be used (Art. 30).

10. *Designation of Genotypes for Genera Published with Identical Limits.*—If two genera with the same limits are formed independently by different authors, without designation of genotypes, any subsequent author may designate the genotypes (Article 30g), and if the types designated are not specifically identical, the two generic names may (other things being equal) be used for restricted genera containing the types in question (Art. 30).

11. *The Designation of Genotypes by Latreille, 1810.*—The "Table des genres avec l'indication de l'espèce qui leur sert de type," in Latreille's (1810) "Considérations générales," should be accepted as designation of types of the genera in question (Art. 30).

12. Of no entomological interest.

13. Of no entomological interest.

14. Of no entomological interest.

15. (In part). Presentation of a paper before a scientific society does not constitute publication in the sense of the Code.

16. *The Status of Prebinomial Specific Names (Published Prior to 1758) under Art. 30d.*—In deciding whether a case of absolute tautonomy is present (under Art. 30d), the citation of a clear prebinomial specific name in synonymy is to be construed as complying with the demands of Art. 30d. Examples: *Equus caballus* (*Equus* cited in synonymy in the sense of "the horse"), *Alca torda* (*Alca* cited in synonymy in the sense of "the Alca").

17. *Shall the Genera of Weber, 1795, be Accepted?*—Weber's *Nomenclator Ento-*

mologicus, 1795, complies with the requirements of Article 25, hence the genera in question are to be accepted, in so far as they individually comply with the conditions of the Code.

18. Of no entomological interest.

19. Of no entomological interest.

20. *Shall the Genera of Gronow, 1763, be Accepted?*—Gronow, 1763, is binary, though not consistently binominal. Article 25 demands that an author be binary and Article 2 demands that generic names be uninominal. Under these Articles, Gronow's genera are to be accepted as complying with the conditions prescribed by the Code to render a name available under the Code. [Cf. 89.]

21. *Shall the Genera of Klein, 1744, Reprinted by Walbaum, 1792, be Accepted?*—When Walbaum, 1792, reprinted in condensed form (but did not accept) the genera of Klein, 1744, he did not thereby give to Klein's genera any nomenclatorial status, and Klein's genera do not therefore gain availability under the present Code by reason of being quoted by Walbaum.

22. Of no entomological interest.

23. Of no entomological interest.

24. ANTENNARIUS *Commerson, 1798, and Cuvier, 1817, vs.* HISTRIO *Fischer, 1813.—Antennarius* Commerson is a uninominal generic name (Art. 2) of an author who used a binary (Art. 25) (though not binominal) nomenclature. It received nomenclatorial status by virtue of its publication by Lacépède, 1798, and should date from that time instead of from Cuvier, 1817. It is therefore not necessary to suppress it in favor of *Histrio,* 1813. [Cf. 89.]

25. DAMESIELLA *Tornquist, 1899, vs.* DAMESELLA *Walcott, 1905.*—Under Article 36, Recommendations, it is not necessary to reject *Damesella,* 1905, because of the existence of *Damesiella,* 1898 (1899?).

26. Of no entomological interest.

27. Of no entomological interest.

28. *Shall the Nouvelle Classification of Meigen, 1800, be Given Precedence over Meigen's Versuch, 1803?*—The generic names contained in Meigen's "Nouvelle Classification," 1800, must take precedence over those in his "Versuch," 1803, in every case where the former are found valid under the International Code.

29. PACHYNATHUS *vs.* PACHYGNATHUS.—On basis of argument in Opinion 26, and in view of the prior name *Pachygnathus,* 1834, Arach., the Commission is of the opinion that *Pachynathus* Swainson, 1839, should be suppressed.

30. Of no entomological interest.

31. Of no entomological interest.

32. *The Type of the Genus* SPHEX.—On the basis of the premises submitted, *sabulosa* is the type of *Sphex* Linnaeus, 1758.

33. Of no entomological interest.

34. ÆSCHNA *vs.* ÆSHNA.—Since evidence of the derivation of the word is not contained in the original publication, the original spelling of *Æshna* should be preserved.

35. *Types of Genera of Binary but not Binominal Authors.*—In determining the type of a genus, the selection must be confined to species included under the generic name in question at the time of its original publication, regardless of the fact whether they were named binominally or not. If, however, a generic name is distinctly proposed as a substitute for an earlier generic name, the species of the latter are to be taken into consideration.

36. Of no entomological interest.

37. Of no entomological interest.

38. Of no entomological interest.

39. *On the Status of the Latin Names in Cuvier, 1800.*—The Latin names in the systematic tables given in Cuvier, 1800 (*Leçons d'anatomie comparée*), are available in so far as they are identifiable through the bibliographic references given on page xix of the introduction.

40. Of no entomological interest.

41. Of no entomological interest.

43. *On the Status of Genera the Type Species of Which Are Cited without Additional Description.*—When the characters given for a genus cover the genus and the type species, both the genus and the type species are published in the sense of the Code.

44. Of no entomological interest.

45. Of no entomological interest.

46. *Status of Genera for which no Species was Distinctly Named in the Original Publication.*—In genera published without mention, by name, of any species, no species is available as genotype unless it can be recognized from the original generic publication; if only one species is involved, the generic description is equivalent to the publication of "*X-us albus,* n.g., n. sp."; if several species are referred to but not mentioned by name, one of these species must be taken as type; if (as in *Aclastus* Foerster, 1868) it is not evident from the original publication of the genus how many or what species are involved, the genus contains all the species of the world which would come under the generic description as originally published, and the first species published in connection with the genus (as *Aclastus rufipes* Ashmead, 1902) becomes *ipso facto* the type.

47. Of no entomological interest.

48. Of no entomological interest.

49. SIPHONOPHORA ASCLEPIADIFOLII *vs.* NECTAROPHORA ASCLEPIADIS.—On basis of the data submitted, *asclepiadifolii* Thomas, 1879, stands in preference to *asclepiadis* Cowen, 1895.

50. APHIS AQUILEGIAE FLAVA *vs.* APHIS TRIRHODA.—Since the name *Aphis aquilegiae flava* Kittel, 1827, is polynomial and is not available under the Code, *Aphis trirhoda* Walker, 1849, is the correct name for this species.

51. Of no entomological interest.

52. [Part]. It is not feasible for the Commission to issue an opinion upon the question: What constitutes an adequate description? The citation of the type locality of a species is not adequate to establish a name under Art. 25*a* of the Code. If specific characters are given in addition to the type locality, the type locality becomes a part of the description and is to be considered as an important element in determining the identity of the species.

53. Of no entomological interest.

54. Of no entomological interest.

55. Of no entomological interest.

56. Of no entomological interest.

57. Of no entomological interest.

58. Of no entomological interest.

59. Of no entomological interest.

60. Of no entomological interest.

61. Of no entomological interest.

62. *Type Species of Other Genera Are Not Excluded from Consideration in the Selection of the Type of a Genus.*—Article 30 does not exclude the type species of

other genera from consideration in the selection of the type of a given genus. [Part, remainder of no entomological interest.]

63. Of no entomological interest.

64. *Serial Letters, as a, b, c, etc., Are Not Acceptable as Specific Names.*—Such letters are not to be considered as proper specific names.

65. *Case of a Genus Based upon Erroneously Determined Species.*—If an author designates a certain species as genotype, it is to be assumed that his determination of the species is correct; if a case presents itself in which it appears that an author has based his genus upon certain definite specimens rather than upon a species, it would be well to submit the case, with full details to the Commission. At the present moment, it is difficult to lay down a general rule.

66. Of no entomological interest.

67. Of no entomological interest.

68. Of no entomological interest.

69. Of no entomological interest.

70. *The Case of* LIBELLULA AMERICANA *L., 1758, vs.* LIBELLULA AMERICANUS *Drury, 1773.*—In view of the fact that *Libellula americanus* Drury, 1773, is an evident *lapsus calami* for *Gryllus americanus,* the *lapsus* is to be corrected, and the specific name in this instance, *americanus* 1773, is not invalidated by *Libellula americana* 1758.

71. *Interpretation of the Expression "Typical Species" in Westwood's (1840) Synopsis.*—The species cited by Westwood, 1840 (An Introduction to the Modern Classification of Insects, vol. 2, Synopsis, separate pagination, pages 1 to 158), as "typical species" are to be accepted as definite designations of genotypes for the respective genera. The question whether any given species under consideration represents the valid genotype or not is dependent upon two points: *First,* whether the species was available as genotype, and, *second,* whether this designation in 1840 is antedated by some other designation.

72. *Herrera's Zoölogical Formulae.*—Designations of animals, according to the system proposed by Herrera, in the case submitted for opinion, are formulae, and not names. Accordingly they have no status in nomenclature, and are therefore not subject to consideration under the Law of Priority. No author is under obligation to cite these designations in any table of synonymy, index, or other list of names.

73. (Part). *Eight Generic Names in* ACARINA, *Placed in the Official List of Generic Names.—Amblyomma, Argas, Dermacentor, Haemaphysalis, Hyalomma, Ixodes, Rhipicentor, Rhipicephalus.*

74. *Apstein's (1915) List of Nomina Conservanda.*—The Commission has no power to adopt *en bloc* Apstein's list of proposed Nomina Conservanda, but is prepared to consider names separately upon presentation of reasonably complete evidence.

75. Of no entomological interest.

76. Of no entomological interest.

77. Of no entomological interest.

78. Case of DERMACENTOR ANDERSONI *vs.* DERMACENTOR VENUSTUS.—On basis of the premises presented, the Commission is of the opinion that *Dermacentor venustus* dates from Marx in Neumann, 1897, type specimen Collection Marx No. 122 (U.S. National Museum), from *Ovis aries, Texas,* and that *Dermacentor andersoni* dates from Stiles, 1908, holotype U.S.P.H. & M.H.S. 9467, from Woodman, Montana.

79. *Case of Lamarck's (1801a) Système des Animaux sans Vertèbres.*—"Rigidly

construed," Lamarck's (1801a) *Système des Animaux sans Vertèbres* is not to be accepted as designation of type species.

80. Of no entomological interest.

81. *The Genotype of* CIMEX, ACANTHIA, CLINOCORIS, *and* KLINOPHILOS.—On basis of the premises before the Commission, the common bedbug of Europe, *Cimex lectularius,* is the genotype for *Cimex* 1758, *Acanthia* 1775, *Clinocoris* 1829, and *Klinophilos* 1899 (*Clinophilus* 1903) and its proper designation under the Rules is *Cimex lectularius. Cimex* Linn., 1758, type *C. lectularius,* is hereby placed in the Official List of Generic Names.

82. *Suspension of Rules for* MUSCA *Linnaeus, 1758a, type* M. DOMESTICA.—By authority of the power conferred on the Commission by the 9th International Congress of Zoölogy to suspend the Rules as applied to any given case where in its judgment the strict application of the Rules will clearly result in greater confusion than uniformity, Article 30 is hereby suspended in the case of *Musca* Linnaeus, 1758, and *Musca domestica* Linnaeus, 1758, is hereby designated as type of *Musca,* without prejudice to other cases.

83. Of no entomological interest.

84. Of no entomological interest.

85. Of no entomological interest.

86. Of no entomological interest.

87. *The Status of Proof Sheets in Nomenclature.*—Printer's proof sheets do not constitute publication and, therefore, have no status under the International Rules of Zoölogical Nomenclature.

88. OTARION DIFFRACTUM *vs.* CYPHASPIS BURMEISTERI.—The name of a species is not disqualified merely because the author included in his conception bodily parts of more than one species. The name of a genus based on such a species is therefore available. *Otarion diffractum* Zenker is valid. *Otarion* is to be preferred to *Cyphaspis,* and *C. burmeisteri* Barr. is a synonym of *O. diffractum.*

89. Of no entomological interest.

90. Of no entomological interest.

It is not the writer's intention to enter into a general discussion of the principles of the Rules of Nomenclature, but there are certain matters to which it may be well to refer.

It will be noted that in the International Code no consideration is given to the names of groups higher than the family, nor is any mention made of one well-recognized group lower than the family—the tribe. There is no provision for establishing the proper names of orders, nor is anything said concerning superfamilies. In certain other respects also the Code is deficient, notably in its dealing with the matter of specific types.

It was, perhaps, partially because of these deficiencies that two entomologists, Nathan Banks and A. N. Caudell, prepared and issued—privately printed—in 1912 a code for the use of entomologists.[5] While this code has no official status and consequently *should not be followed in any matters where it may conflict with the International Code,* it was well-

[5] Banks, N., and Caudell, A. N., *The Entomological Code,* Washington, 1912.

considered and in many respects is much more detailed and complete than the International Code. Where it serves to amplify points which are not clear, or where it provides for matters concerning which the official Code is silent it would be well to follow it. It would be entirely impractical to reprint it here in full, but the student who is seriously interested in questions of nomenclature is urged to secure a copy.

The question of type designations for species is considered at some length, whereas in the International Code it is dismissed with a bare recommendation. This is an extremely important point and care in connection with it should be used by every systematic student. The proper designation of types can be made a most useful medium for preventing errors and confusion. It should be remembered that the actual type is the court of last resort in settling questions of identity. A very large part of our present systematic work will depend absolutely on types for its final clarification and even if work were done in full accord with the principles which have earlier been expressed in this volume the type would still remain the final arbiter and the final check on error.

Because of the importance of the matter and the extensive treatment accorded it by the Banks and Caudell Code those portions of the code pertaining to it are here included.

THE TYPE OF A SPECIES

70. The type of a species is a single specimen. Specimens other than the type used in making the original description are termed cotypes or paratypes; cotypes when no type is designated, paratypes when a type is indicated. Many other names are in use for certain identified specimens. A number of these are as follows:

Topotype.—A specimen from the original locality.

Metatype.—A specimen compared by the author of the species with the type and determined by him as conspecific with it.

Homotype.—A specimen compared by another than the author of a species with the type and determined by him as conspecific with it.

Holotype.—A single specimen selected by the author of a species as its type, or the only specimen known at the time of description.

Syntype.—One of two or more specimens upon which a species is based when no holotype has been selected (= cotype).

Lectotype.—A syntype selected as a holotype.

Allotype.—A paratype of the opposite sex to the holotype.[6]

[6] In the absence of any authority competent to rule upon this point the writer would consider that the term "allotype" might be applied to any individual upon which the description of the sex opposite the holotype is based, regardless of time or author. The definition given above would preclude the designation of an allotype unless both sexes were in the author's hands at the time of the original description, an unreasonable restriction.

Monotype.—The holotype of a species based on a single specimen.

Chirotype.—The type of a manuscript name.

Plesiotype.—A specimen upon which subsequent or additional description or figure is based.

Apotype = Plesiotype.

Hypotype = Plesiotype.

Neotype.—A plesiotype selected to represent the holotype when the holotype is lost or destroyed.

Heautotype.—A specimen named by the author of a species, or one used by him for figuring. Practically a metatype.

Plastotype.—A cast of a type.

Morphotype.—The type of the dimorphic form of a species.

Autotype = Heautotype.

Homeotype = Homotype.

Ideotype.—A metatype, but a specimen not from the type locality.

Androtype.—Male type; Gynetype—female type.

Various combinations of the above have been used and many other names could be given for particular conditions in entomology, as for specimens from the same nest, as the type of social species, the descendants of the type of a species described from living specimens, from the same batch of eggs, from the same host, etc.

71. In the case of Coccids or other insects where the description is based on specimens in the natural condition, and on ones mounted on slides, the specimens in the natural condition may be considered as type material, and the slide (or one of the slides if there are more than one) as the type slide, but in case of doubt, or a mixture of species, the type slide shall retain the name.

72. In case of minute insects or acari where more than one specimen is mounted on a slide one should label but one slide as the type slide, and if there is doubt or a mixture of species it shall be treated as a case where the author has labeled more than one specimen as type.[7]

73. The following rules apply for the fixation of types of species in the order given.

A. AUTHOR'S FIXATION IN THE ORIGINAL DESCRIPTION

a) If a specific name is based on but one specimen, that specimen is the type.

b) If an author labels or, in the original description, designates a certain specimen as type, that specimen is the type.

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[7] With certain of the implications of this paragraph the author disagrees entirely. Although it has been a common practice in the case of minute forms to mount several or even many specimens on a single slide, such a procedure is as indefensible as it would be to mount many large specimens upon a single pin and to label them all type. The size of the specimens is not in any way a pertinent factor in determining nomenclatorial practice. *In the case of new species based on slide material there should be selected a single individual to serve as holotype, preferably an individual which is mounted separately; but lacking this the type should be clearly indicated on the label.*

c) If an author in the original description designates a series or collection of specimens as type, then the type is among such designated material.

Note.—Obvious errors or transposals of labels are sufficient grounds for disregarding rules *b* and *c*.

B. FIXATION BY OTHERS THAN THE AUTHOR, OR BY THE AUTHOR SUBSEQUENT TO THE ORIGINAL DESCRIPTION

d) A specimen to be chosen as type must be found among material presumably examined by the author at the time of its description.

e) The type shall not be selected from among material indicated in the original description as variational or aberrational, nor from material doubtfully included in the species.

f) If it is anywhere stated in a work that a new species described therein is based on material wholly from a source other than the author's own collection, the type is to be selected from among such material.

g) If there are distributed to two or more collections specimens of a species all labeled by the author as types, or none labeled as types, the type should be selected from the specimens retained, if any, in the collection of the author. In case of a joint work the first author has precedence.

h) If two or more species are included in the original series of specimens upon which a new species has been based and the author has not labeled, nor has he nor anyone else designated one as type, nor restricted the type material, and it results that one of the included species is a synonym of an older valid species or has been afterwards (knowingly or unknowingly) described as a new species, then the type is restricted to the remaining material.

Example.—*Alpha alba* Latr. 1831, described without type fixation from two specimens, *a* and *b*, which prove to represent two species, *a* proving to be the *Alpha brunnea* of Linn. 1758. Thus the type of *Alpha alba* Latr. is the specimen *b* by restriction according to the above rule. The same would have resulted had the species *a* been described as new prior to the type designation of *alba*.

i) The first intentional definite type designation in accord with the above rules is final. The mere reference to a specimen as type is not considered as type fixation.

74. Where the type of a species is lost, or destroyed, the first interpretation shall obtain unless later acquired information clearly proves it should be otherwise, when a change is allowable.

75. In cases where specimens have been labeled as types by others than the author of the species such type labels shall be interpreted independently by each investigator since there is much variation in the credibility of such labels.

76. A species based wholly on a figure has the original of that figure as the type.

77. The type of a specific name proposed to replace a preoccupied specific name is the same as the type of the name replaced, irrespective of any attached description.

78. The type of a new name proposed to replace a misidentification is among the material in the collection of the author of the new name, if there is inherent evidence that he had material and he gives a description or figure; otherwise, if based on the misidentification of a specified author the type is among the material utilized by such specified author if there is inherent evidence that he had material. If the name is given for the misidentification of several or no specified authors, then the type may be selected by any subsequent worker from among the material of the authors concerned. If it results that there is no described material back of a name based on a misidentification or misinterpretation then such name is invalid.

The International Code provides only for the formation of family and subfamily names and hints at type genera for them (Arts. 4, 5) but it makes no provision for any other of the higher categories and does not indicate how the type genera shall be selected.

The Banks and Caudell Code considers these matters more fully in the following sections.

108. The name of a family shall be formed by changing the last syllable of the genitive case of an included generic name (preferably the oldest) into *idae*.

109. The name of a subfamily shall be formed by using *"inae"* in place of the *"idae."* One of the subfamily names shall be based on the same generic name as that of the family of which the subfamily is a part.

110. The name of a superfamily shall be formed by replacing the *"idae"* of one of the included family names with *"oidea."*

111. The name of a tribe (a prime division under the subfamily) shall be based on that of an included genus, and shall end in *"ini."* One of the tribes under a subfamily shall be based on the same genus as that of the tribe of which it is a part.

Note.—The term "group" formed by adding *"ae"* to the stem of a genus, is used by many under the tribe, subtribe, or subfamily, but the term is used also for other grades and is inadvisable owing to this confusion.[8]

113. The name of a family, or subfamily is to be changed when the basic generic name is a homonym, or is removed from the family or subfamily, or becomes a synonym.

114. If there are two or more names proposed for the same family or subfamily ending in *idae* or *inae,* the earlier name shall be adopted.

115. If there are two family or subfamily names of the same spelling, the more recent shall be replaced, or so modified as not to conflict.

All of this is much more explicit than the International Code, but even yet it is not entirely satisfactory. The writer would suggest the following additions and modifications:

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[8] For this category the term "supergenus" might well be used, with this ending. G. F. F.

Every subdivision of each category higher than the genus, up to and including the superfamily, shall have a type genus, and the names of these subdivisions shall in every case be formed by adding the proper endings to the name of the type genus.

In every category one member must have the same type genus as the including member of the next higher category; i.e., of all the supergenera contained in a subtribe one must have the same type genus as does the including subtribe; of all the subtribes contained in a tribe one must have the same type genus as the including tribe; and so on.

The type genus of every category shall be that upon which the name of that category was first based, subject to the preceding provision and to changes made necessary by synonymy and homonymy. The name and status of every category shall follow and depend upon the name and status of the type genus.

In the naming of any member of these higher categories it may be preferable to select the oldest included genus as type, but if this is not done the name of the member shall not be changed because of that fact. The rule of priority should apply throughout.

In the restricting, dividing, or uniting of members of these categories the same general procedure should be followed as in the case of genera.

The International Code makes no provisions whatsoever concerning names above the superfamily. The Banks and Caudell Code recommends the application of the rule of priority to such names.

The question of what constitutes "publication" is difficult and the International Code at present gives no aid in its solution. The Banks and Caudell Code considers this at some length. Summarizing the provisions of this latter Code it may be said that to have been validly published a description must actually have been printed (not merely mimeographed) and offered for sale or distribution, and it must have been printed in something other than a newspaper or other non-technical journal. A very illuminating discussion of the whole problem is to be found in a paper by C. W. Stiles,[9] Secretary of the International Commission on Zoölogical Nomenclature, to which the student is referred.

The Banks and Caudell Code deals also with many minor details, which will not be discussed here.

The following terms have been utilized for the characterization of the various kinds of genotypes.

Orthotype, a type by original designation.

Haplotype, a type by single reference (only species).

Logotype, a type by subsequent designation.

Pseudotype, an erroneous type designation.

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[9] Stiles, C. W., "What Constitutes Publication?" *Science* (n.s.), *LXVII*, 471–478, (1928).

CHAPTER XII

THE TRAINING OF THE SYSTEMATIST

The doing of all these things which are involved in the work of the systematist is a technical task which should be undertaken only by those who are technically trained for its accomplishment. The acquirement of this technical training should be considered essential for anyone who wishes to be called a systematist.

We have already called attention to the curious circumstance that systematic work in general has admitted the untrained worker to a position of parity with others and to the influence that this has had upon the quality of our work. It may be emphasized that the question as to whether the systematist be a professional or an amateur is in itself of no weight—the significant factor is that of the adequacy of his training. If our work be of general value and importance (and we believe that it is), if it is to become a dignified and worthy profession (and we believe that it will), then those who participate in it may be expected to fit themselves for such work as carefully and completely as must the members of any other profession.

As to the question of what should be included in the professional training of the systematic entomologist there may be great difference of opinion. Certain parts of his technical knowledge and a certain portion of his equipment of ideas seem very clearly indicated, so clearly that they may safely be discussed.

We have already considered the argument that the systematic entomologist should be adequately trained as a technician in the preparation of his material for study and that he should be adequately prepared as a draftsman. These things need be dealt with no further. But there are certain other details of his technical equipment that may be considered.

One of the most important of all the tools which the systematist must employ is literature. This is particularly the case in this field where our methods require that so much consideration be given to what has been done by others. If a physiologist publishes a paper which proves to cover the same ground that some other paper has covered, nothing is involved but a duplication of effort. There may even be a certain gain by mutual reinforcement. If a systematist does the same there results a confusion of nomenclature that it may take years to clear up. From the nomenclatorial point of view the systematist must be an accomplished bibliographer.

This being the case one of the first things that the student of systematics should do is to become familiar with the sources from which a

knowledge of what has been done can be obtained. It is a part of his business to know such things. The more important of these sources are included in the following list:

The Zoölogical Record, published by the Zoölogical Society of London, attempts to keep a complete record of everything published in zoölogy. Its chief deficiency is that it is always much behind the current literature.

The *Bibliographia Zoologica,* issued by the Concilium Bibliographicum in Switzerland, prepares yearly volumes of essentially the same character as the *Zoölogical Record.*

For current references the student will find the *Review of Applied Entomology,* published by the Imperial Bureau of Entomology in London, an excellent source. The newly established *Biological Abstracts,* issued under the auspices of all the American biological societies, has a similar function. Certain of our journals, notably *Entomological News,* publish a condensed record of current literature. These are sufficient at least to give any student a start upon his essential bibliographic work.

In dealing with generic names it is frequently necessary to know where and when any given name has been used. For this purpose the student will refer to Sherborn's *Catalogus Animalium,* which gives a complete list of all generic and specific names up to 1850; Scudder's *Nomenclator Zoologicus* and its supplement, which aim to give a list of all generic names up to 1882; Waterhouse's *Index Zoologicus,* which gives a list of generic names as recorded in the *Zoölogical Record* from 1880 to 1910.

Connected with bibliographic work is the making of catalogues. They are among the most useful of tools, and one of the greatest deficiencies of our systematic literature is the paucity of such aids. The making of such catalogues is one of the most important and immediately useful tasks in which the systematist can engage, for if no catalogue exists one of the first things that the student of any special group must do is to make one for himself. It may possibly be that the making of such catalogues involves only a high grade of clerical work, as some assert, but no one entirely without systematic training could produce a thoroughly good catalogue. In view of their great importance it may not be out of place to offer some practical suggestions concerning them.

The ideal catalogue should make it possible to locate every species and every other category under every alias or combination by which it may have been known.

The majority of catalogues do not do this and possibly such an ideal is impractical in the case of the larger groups. In the case of small groups, however, it can certainly be attained. We may call attention to a single example that approximates this ideal condition. One of the best catalogues

of a limited group that can be found is that of Harrison for the little order Mallophaga.[1]

Although this deals with a small order there are listed 2,395 specific names, of which 1,520 are treated as valid. It includes a bibliography of all the works dealing with systematics and nomenclature; a list of new names introduced in the publication; a list of the names used in the old "mother genera" from which most of the later genera were derived by nomenclatorial splitting, together with the present disposition of these names; a list of generic and subgeneric names, valid and invalid, that had been used for Mallophaga up to the time of its preparation; a classification of the group to subfamilies; and the list of specific names.

In the list of specific names each species is listed under every genus to which it has been referred, except where the genus itself is a synonym. By the use of a varied typography—unfortunately rather costly—the status of the various names is clearly indicated. Under each genus there appear three series of names. One, in black face type, is employed for the species belonging to that genus, and is numbered in the same type; the other two appear under the same series of italic numbers, but the names are in two types—ordinary roman for species now transferred to other genera, and italics for synonyms.

The following sample from the catalogue will illustrate the system:

Genus 9. Menopon.

1. **abdominale** Piaget, *Pediculines,* 1880, p. 473, Pl. 36, f. 9.

(Perdix (Coturnix) coturnix).

1. albiceps Piaget, see *Myrsidea.*
2. albipes Piaget, see *Myrsidea.*
4. anaspilum Nitzsch, *Zeit. f. ges. Nat.,* 1866, p. 119.

(Corvus corax) = *gonophaeum* Nitzsch.

The only addition that might be suggested is that genera which are synonyms might also be listed with the species which have been referred to them. It is questionable, however, whether any such refinement is at all necessary. A comparison of this catalogue with others will show how far it excels the ordinary forms.

It should be assumed that every systematist will have occasion to publish the results of his investigations. This being the case, familiarity with the technique of preparing scientific papers for publication should be a

[1] Harrison, L. A., "The Genera and Species of Mallophaga," *Parasitology,* IX, 1–156, (1916).

part of his equipment. It should be remembered that most of our entomological journals are by no means wealthy. Space in them is limited, and the funds with which to print scientific papers are all too small. A badly prepared manuscript increases printing costs and adds to the labors of editors, most of whom give their services without compensation for the advancement of their science. Consequently the authors of scientific papers should consider it a part of their duty to prepare their papers in such a way as to lighten the labors of editors and save expense—even if there were no other reasons for doing so.

It is not possible here to give any extensive consideration to this subject. Different journals have different standards, and there are available books in which the whole subject is treated at length.[2] The student should become acquainted with these. However, practically every journal has its own peculiarities and in preparing a paper for any special journal its pages should be consulted for samples of its form.

There are many other details of the systematist's work, but this volume is not presented as a handbook of detailed method and they may be passed over. There are, however, certain general ideas that should be emphasized.

If systematic work be in its broader implications essentially synonymous with the study of organic evolution, and indeed if it is to be regarded as at all a part of biology, it would seem to follow that systematists in general should be familiar with general biological theory. They should be in fact biologists and not merely students of special groups, working entirely without regard to anything beyond the limits of those groups.

Connected with this matter of general interest and general training is that of provincialism as opposed to cosmopolitanism. The criticism has at times been voiced that probably the majority of our systematists—in other fields as well as in entomology—approach their work from a purely local and provincial point of view. The limitation of one's work by purely geographical or political boundaries is entirely inconsistent with the development of any deep understanding, any broad philosophy, or any profound contribution. Better to limit one's self to a single small group and to consider that group as it occurs in the world, than to adopt an order as confined within the boundaries of a state.

Another part of the equipment of the systematist, as indeed of any scientific man, should be the possession of an independent and a critical

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[2] *How to Prepare a Paper for Publication,* Williams & Wilkins Co., Baltimore.

Preparation of Articles for the Journal of Agricultural Research, Department of Agriculture, Washington, D.C., 1914.

A Manual of Style, University of Chicago Press, 1927.

mind. Our systematic work in entomology has suffered from the general lack of keen scientific criticism. In fact work that is the very negation of scientific method, that violates every rule of sound scientific procedure, has not only been uncritically accepted but effusively praised. There is, to be sure, no place in our scientific literature for personalities, but there is a place and a need for scientific criticism.

There is one more idea, which it seems to the writer, should also be established as a part of the systematist's general equipment. That is, the idea of productivity. It is the aim of research to add to the sum total of human knowledge. It is this goal that gives to our work, in the final analysis, its justification and that accords it dignity. There are involved in productive research two things. One is the doing of the research. The other is the publication of the results. The first without the second is a sterile plant. Knowledge, to become of more than potential value—to become dynamic—must circulate.

It may not be out of place, by way of conclusion, to enumerate a few of the tasks that are still before the students of systematic entomology, to indicate something of the magnitude of the field in which the entomological systematist is engaged.

We have already called attention to the fact that a very large part of what has been done stands in need of revision or even of complete redoing. The clearing up of the existing mass of named species is as important, or even more so, than the naming of more. The strengthening of the existing foundation is essential to the building of a greater structure. What is needed above all is the revision of groups and the redescription of named species. There are needed catalogues of the named forms, manuals, and handbooks. If we had for every group a book as useful as Williston's *North American Diptera* we might consider ourselves fortunate indeed.

Consider for a moment the magnitude of the task that still confronts the students of insects. Estimates as to the possible number of insect species in the world vary, but all of them are large. It is highly improbable that even as many as ten per cent of all the existing species have been even named, and on this basis there may be five million or more species. Consider that of these five million possible species, there are usually two sexes differing appreciably in structure. This means then that there will be ten million adult insect forms, even if we leave out of the count subspecies, races, and the like. Consider further that each of these species passes through a number of immature stages, each of which is likewise distinct. If we assume for purposes of estimate that there are three stages for each species we have fifteen million different immature stages. Totaling it all up we have twenty-five million insect forms! Bear in mind that of these

twenty-five million forms but a small percentage of the adults are even named. Remember also that it is a part of the task of systematists eventually to deal with all of them, mature and immature.

In the face of this does it seem unreasonable to contend that the task is one which will demand a high degree of skill, of training, and of understanding? Is it unreasonable to contend that it should be approached with a clear conception of the difficulties and of the necessities? Is it not desirable that its fundamental requirements be understood and its basic methods reduced as far as may be to a definite system? Is it not also a task in which there is room for the best of intellects to engage and one in which any systematic entomologist may be proud to participate?

INDEX

Abbreviations, ruling on, 148
Acanthia, ruling on, 152
Acarina, list of officially adopted names, 151
Adaptation, 125
Adaptive characters in classification, 125
Aeschna vs. *Aeshna,* ruling on, 149
Amateur:
  Contribution to systematic work, 27
  Effect on standards, 27
Antennarius vs. *Histrio,* ruling on, 149
Antonina, as example, 58
Aphis aquilegiae flava vs. *Aphis trirhoda,* ruling on, 150
Appendix to International Rules, 145
Approximations in systematic work, 44
Art, 80

Balsam, as mounting medium, 69
Bateson, W., quoted, 12, 19, 36
Beetle, *see* Coleoptera
Bibliographic sources, 157
Binary authors, ruling on, 149
Binary nomenclature, 138, 149
Binomial nomenclature, 131
Biological control, 15
Biometry, 46
Breeding true, as criterion of species, 43

Catalogues, making of, 159
Categories of classification:
  Above the species, 110, 112
  Below the species, 51
  Theoretical number of, 121
Caustic potash, 70
Chamberlin, J. C., quoted, 72, 114, 116, 119
Chemical definition of species, 50
Chemical studies in systematic work, 48
Chitinized, use of word, 91
Cimex, ruling on, 152
Classification:
  Categories of, 51, 110, 112, 121
  Content of, 108
  Development of, 109
  Difficulties of, 110, 111, 112
  Essence of, 123
  Function of, 109
  General subject, 108
  Nature of scientific system, 110
Clinocoris, ruling on, 152
Coccidae:
  Drawing of, 95
  Examples from, 47, 52, 58, 94, 125, 126
Codes, *see* Nomenclature
Coleoptera:
  Drawing of, 97
  Examples from, 68
  Systematic methods in order, 67
Collections:
  Pinned, 66
  Slide, 73
Color forms, 52
Compound species:
  Division of, 139
  Ruling on, 152
Comstock-Needham system of wing-vein nomenclature, 63
Convenience, as principle in classification, 111
Cosmopolitanism, need of, 161
Criteria for segregation of species, 38, 39
Curve of distribution, 114
Cuvier, ruling on names of, 150

Damesiella vs. *Damesella,* ruling on, 149
Deficiencies of pinned material, 67
Deficiencies of systematic work, 26
Dermacentor andersoni vs. *Dermacentor venustus,* ruling on, 151
Description:
  Adequacy of, ruling on, 150
  Arrangement of, 103
  Content of, 102
  Difficulties of, 76
  Functions of, 76, 100, 101
  General subject, 100
  Standards of, 30
Description of species, 22
Determinations:
  Erroneous, 150
  Importance of, 15
  Specific, 77
Development of:
  Nomenclature, 128, 129
  Systematic work, 26
Diagnosis of species, 30, 31, 101

Difficulties of systematic entomology, 28, 29
Dimorphic forms, naming of, 57
Dimorphism, 52
Diptera, methods in, 69
Diptera Pupipara, 69, 72
Distinctness, as criterion of species, 39, 40
Distribution:
  Curve of, 114
  Problem of systematics, 13
Distribution of organisms, as problem in systematics, 13
Drafting:
  Aim of, 78
  Entomological, 76
  Principles of mechanical, 79
Drawing:
  Equipment, 81
  Methods, 80
Drawings:
  Divided, 84
  Outline, 86
  Preliminary, 83
  Purpose of, 78, 84
  Reproduction of, 81
  Size of, 82
Duty of the systematist, 16

Ecological data in systematic work, 42, 45
Economic entomology and systematic work, 15
Entomological drafting, 76
Entomological Society of Washington, rules of publication, 102
Environmental influences, 51
Erroneous determinations, ruling on, 151
Ethics, Code of, 143
Evolution, 12
Evolution and systematics, 130
Experiment in systematic work, 46

Family names, 135
Ferris, G. F., quoted, 113
Ferris, G. F., and Chamberlin, J. C., referred to, 91
Figures, preparation for publication, 93
Formula of curve of distribution, 116
Formulae in nomenclature, ruling on, 151
Fuchsin, 71

Gage's formula, 71
Gahan, A. B., quoted, 9, 11
Gametic purity, as criterion of species, 50
Genera:
  Division of, 141
  Without included species, 150
  Monotypic, 123
  Names of, 135
  Types of, 141, 157
  Union of, 140
Genetical concept of the species, 51
Genetical methods in systematic work, 4
Genetics and systematics, 14
Genotypes:
  Designation of, 140, 148
  Names of, 157
  Rulings on, 148, 150
  Selection of, 140
Genus:
  A concept, 108, 121
  Changing concept of, 109, 117
  Division of, 141
  Name of composite, 148
  Standard of, 121
Geographic names, transliteration of, 146
Glycerin jelly, as mounting medium, 70
Greek words, transliteration of, 146
Gronow, ruling on genera of, 149

Hall, H., and Clements, F. E., quoted, 37, 39, 43, 45, 49, 50
Harrison, L., quoted, 76, 103, 160
Heinrich, Carl, quoted, 16, 21, 28, 33, 74
Histological studies in systematic work, 49
Homology:
  Basic principle in systematics, 63
  Defined, 63
  In wing veins of insects, 62, 64
Homonyms:
  Defined, 143
  Rejection of, 144
Human origins, problem of systematics, 14
Hybridism, as criterion of species, 42, 43
Hybrids, nomenclature of, 138

Identification of species:
  Difficulties of, 25, 32
  Place in systematic work, 24
  Practical importance, 15

Illustrations, function of, 78
Indication, meaning of, 147
Inter-gradation:
  Criterion of subspecies, 53
  Lack of, as criterion of species, 40
  Problem of, 40, 41
Internal structures, drawing of, 93
International Commission on Zoölogical Nomenclature, 133
International Rules of Nomenclature, 135
Island forms, disposition of, 54

Kermes, as example, 58
Keys, 105
Klein, ruling on genera of, 149
Klinophilos, ruling on, 152

Lamarck, ruling on names of, 151
Languages, 145
Latin in nomenclature, 130, 135
Lecanium, problem of species in, 47
Letters as specific names, ruling on, 151
Libellula americana vs. *Libellula americanus,* ruling on, 151
Lines in drawing, 87
Linnaeus:
  Concept of genus, 109
  Inventor of nomenclatorial system, 130

McAtee, W. L., quoted, 40, 44, 52, 54, 55, 125, 127
McLeod, Julian, quoted, 25, 32, 50
Magenta, 71
Mallophaga, illustrative material from, 91, 103, 113
Manuscript names, status of, 147
Medium of expression in drawing, 81
Meigen, ruling on names of, 149
Methods:
  Description of species, 100
  Drawing, 83
  Mounting Pseudoscorpions, 72
  Preparation of material, 66
Misidentifications in nomenclature, 143, 156
Monotypic genera, 123, 141
Morphology:
  Aims of, 64
  Basis of systematics, 61, 121
  Defined, 61, 62
  Furnishes fixed datum plane, 65
  Indication of degree of relationship, 60
  Limitations of, 57
  Scope of, 61, 63
Mounting media, 69, 70
Musca domestica, ruling on, 152
Mutations, 52, 56, 59
Muttkowski, R. A., quoted, 33

Names:
  Authors', 139
  Derivation of, 148
  Endings of, 148
  Family and subfamily, 135
  Formation of, 138
  Generic and subgeneric, 135
  Manuscript, 147
  Orthography of, 138
  Prebinomial, 148
  Pre-Linnean, 148
  Published in 1758, 147
  Rejection of, 145
  Specific and subspecific, 136
  Validation of, 139
Naming of species:
  Aim of, 56
  Value of, 20
Nomenclature:
  A.O.U. Code, 133
  Banks and Caudell Code, 152
  Blanchard Code, 133
  Dall Code, 133
  Development of, 128, 129
  Douvillé Code, 133
  Function of, 21
  International Code, development of, 133
  International Commission on, 147
  International Rules, 135
  Linnean system of, 130
  Opinions of International Commission on, 147
  Stricklandian Code, 132
  Zoölogical, 135
Nomina Conservanda, ruling on, 151
Nuttall, studies on *Pediculus,* 48

Orthography of zoölogical names, 138
Otarion diffractum vs. *Cyphaspis burmeisteri,* ruling on, 152

Pachynathus vs. *Pachygnathus,* ruling on, 149
Paleontology, a problem of systematics, 13
Pearl, Raymond, quoted, 9, 11, 12, 17
Pediculus, example from, 48
Photographs, 80
Phylogenetic method in taxonomy, 45
Phylogenetic stocks, as species, 50
Phylogeny in classification, 114, 120
Physiological species, 48
Pinned material, limitations of, 66
Polymorphic forms, naming of, 57
Polymorphism, 52
Practical application of systematics, 14
Practical considerations in systematic work, 37
Preparation of figures for publication, 93
Preparation of material:
  General subject, 66
  Large insects, 74
  Mounting on slides, 74
  Pinned material, 66
  Slide preparations, 69
  Staining, 71
Preserved material, limitations of, 37, 39
Principles of segregation of species and subspecies, 56
Principles of systematic entomology, 33
Priority:
  Application of, 140
  Law of, 139
Proof sheets, ruling on, 152
Pseudococcus, example from, 52
Pseudoscorpions, preparation of, 72
Publication, 140, 160
  Definition of, 148
  Questions concerning, 157

Redescription of species, 105
Rejection of names, 145
Reproduction of figures, 81
Restriction:
  Genera, 141
  Species, 142
Rules of Nomenclature:
  International, 135
  Place in systematics, 21
  Suspension of, 144
  Suspension of, in case of *Musca domestica,* 152

Säurefuchsin, 71
Scientific names, 135
Sclerotic, use of word, 91
Sclerotic areas, drawing of, 91
Segregation of species:
  Criteria for, 38, 39, 40
  First step in systematics, 24
  Importance in systematics, 36
Setae, drawing of, 89
Sexual characters, 125
Shading in drawing, 87
Similarity in names, 144
Siphonophora asclepiadifolii vs. *Nectarophora asclepiadis,* ruling on, 150
Species:
  Central problem of systematics, 19
  Chemical entity, 50
  Concept or entity, 49
  Definition of, 50, 57
  Description of, 100
  Division of, 139, 142
  Gametic concept of, 51
  Kinds of, 36
  Phylogenetic stock, 50, 51
  Physiological, 48
  Problem of, 20
  Segregation of, 24, 36
  Transference of, 139
  Types of, 153
Specific names, 136
Spelling of names, 137, 138, 148
Sphex, type of, 149
Stains, 71
Standards:
  Genus, 121
  Systematic entomology, 24
  Systematic work, 27, 28, 29
Statistical methods in systematic work, 45
Stiles, C. W.:
  Quoted, 133
  Referred to, 157
Subfamily names, 135, 156
Subgeneric names, 135, 136
Subspecies:
  Definition of, 53, 57
  Names of, 136, 138
Subspecific names, 136, 138
Superfamily names, 156
Suspension of rules, 144

Suspension of rules in case of *Musca domestica,* 152
Synonym defined, 143
Systema Naturae, 130, 140
Systematics:
  Beginnings of, 10
  Democracy of, 27, 28
  Place in biology, 11
  Principles of, 26, 33
  Quality of, 25
  Scope of, 18, 22
Systematist:
  Contribution to biology, 9
  Duty of, 16
  Task of, 162
  Training of, 158

Tautonymy, 141, 143
Taxonomy, 22
Terzi, A. J. E., referred to, 98
Theory of Evolution, 12
Training of the systematist, 158
Transfer of species, 139
Tribe, name of, 156
Type fixation:
  Author, 154
  Other than author, 154
Types:
  In Banks and Caudell Code, 153
  Categories above genus, 157
  Fixation of, 154
  Generic, 140, 141, 150, 157
  Importance of, 153
  Nomenclature of, 145, 153, 157
  Specific, 145, 153
Typical species, ruling on, 151

Union of genera, 140
Union of species, 140
Unity of life, 12

Variation, 38, 52
Variety, 54, 155
Venation of wings, 62, 64

Weber, genera of, 148
White, Andrew D., referred to, 12
Willis, J. C., referred to, 114

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